Vol. 43, No. 2 March, 1936

PSYCHOLOGICAL REVIEW PUBLICATIONS

Psychological Review

EDITED BY

HERBERT S. LANGFELD, PRINCETON UNIVERSITY

S. W. FERNBERGER, UNIV. OF PENNSYLVANIA (*J. of Exper. Psychol.*)
W. S. HUNTER, CLARK UNIVERSITY (*Psychol. Index*)
JOHN A. McGEOCH, WESLEYAN UNIVERSITY (*Psychol. Bull.*)
JOHN F. DASHIELL, UNIVERSITY OF NORTH CAROLINA (*Psychol. Monog.*)

CONTENTS

The Vertical Dimensions of Mind: JOHN A. McGEOCH, 107.

The Postulates of Psychology: JOHN E. WINTER, 130.

Studies in Cross Education. V. Theoretical: THOMAS W. COOK, 149.

Intercorrelations of Measures of Human Learning: CALVIN S. HALL, 179.

PUBLISHED BI-MONTHLY
FOR THE AMERICAN PSYCHOLOGICAL ASSOCIATION
BY THE PSYCHOLOGICAL REVIEW COMPANY
PRINCE AND LEMON STS., LANCASTER, PA.
AND PRINCETON, N. J.

Entered as second-class matter July 13, 1897, at the post-office at Lancaster, Pa., under Act of Congress of March 3, 1879

PUBLICATIONS

OF THE

AMERICAN PSYCHOLOGICAL ASSOCIATION

EDITED BY

S. W. FERNBERGER, University of Pennsylvania (*J. Exper. Psych.*)
WALTER S. HUNTER, Clark University (*Index and Abstracts*)
HENRY T. MOORE, Skidmore College (*J. Abn. and Soc. Psychol.*)
HERBERT S. LANGFELD, Princeton University (*Review*)
JOHN A. McGEOCH, Wesleyan University (*Bulletin*)
JOHN F. DASHIELL, Univ. of North Carolina (*Monographs*)

HERBERT S. LANGFELD, Business Editor

PSYCHOLOGICAL REVIEW

containing original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 540 pages.

PSYCHOLOGICAL BULLETIN

containing critical reviews of books and articles, psychological news and notes, university notices, and announcements, appears monthly (10 numbers), the annual volume comprising about 720 pages. Special issues of the Bulletin consist of general reviews of recent work in some department of psychology.

JOURNAL OF EXPERIMENTAL PSYCHOLOGY

containing original contributions of an experimental character, appears bi-monthly, February, April, June, August, October, and December, the six numbers comprising a volume of about 900 pages.

PSYCHOLOGICAL INDEX

is a compendious bibliography of books, monographs, and articles upon psychological and cognate topics that have appeared during the year. The Index is issued annually in June, and may be subscribed for in connection with the periodicals above, or purchased separately.

PSYCHOLOGICAL ABSTRACTS

appears monthly, the twelve numbers and an index supplement making a volume of about 700 pages. The journal is devoted to the publication of non-critical abstracts of the world's literature in psychology and closely related subjects.

PSYCHOLOGICAL MONOGRAPHS

consists of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The price of single numbers varies according to their size. The Monographs appear at irregular intervals and are gathered into volumes of about 500 pages.

JOURNAL OF ABNORMAL AND SOCIAL PSYCHOLOGY

appears quarterly, June, September, December, March, the four numbers comprising a volume of 448 pages. The journal contains original contributions in the field of abnormal and social psychology, reviews, notes and news.

ANNUAL SUBSCRIPTION RATES

Review: $5.50 (Foreign, $5.75). **Index:** $4.00 per volume.
Journal: $7.00 (Foreign, $7.25). **Monographs:** $6.00 per volume (Foreign, $6.30).
Bulletin: $6.00 (Foreign, $6.25). **Abstracts:** $6.00 (Foreign, $6.25).
Abnormal and Social: $5.00 (Foreign, $5.25). Single copies $1.50.
Current numbers: Journal, $1.25; **Review,** $1.00; **Abstracts,** 75c; **Bulletin,** 60c.

COMBINATION RATES

Review and Bulletin: $10.00 (Foreign, $10.50).
Review and J. Exp.: $11.00 (Foreign, $11.50).
Bulletin and J. Exp.: $12.00 (Foreign, $12.50).
Review, Bulletin, and J. Exp.: $16.00 (Foreign, $16.75).
Review, Bulletin, J. Exp., and Index: $19.00 (Foreign, $19.75).

Subscriptions, orders, and business communications should be sent to the

PSYCHOLOGICAL REVIEW COMPANY

PRINCETON, NEW JERSEY

VOL. 43, NO. 2 MARCH, 1936

THE PSYCHOLOGICAL REVIEW

THE VERTICAL DIMENSIONS OF MIND[1]

BY JOHN A. McGEOCH

Wesleyan University

INTRODUCTION

The problems of psychology may be studied either cross-sectionally or vertically. The distinction between the two modes is, of course, not absolute. Cross-sectional attack implies a description of psychological phenomena at a given moment or over a relatively brief span of time. This span may be extended theoretically until cross-section and long-section are indistinguishable, but in experimental practice it seldom is. When the span is thus extended, the data may still be *interpreted* as data bearing only upon the particular period studied, *i.e.*, cross-sectionally, or as aspects of a continuous and longer flow of events, *i.e.*, vertically.

The earlier history of experimental psychology shows a domination by cross-sectional analysis. The search was for what mind is at a given moment or at a given brief temporal span. Even when the problems were stated functionally they were cross-sectional. They sought the reactive status of the organism at a given time, not the flow of events and conditions by which a given status had been reached. The rise of a biologically oriented functionalism extended the temporal cross-section, but the emphasis was still on the adaptive significance of an act in a particular situation. The dominant systematizing aimed at a classification of the conscious com-

[1] Address of the president before the Midwestern Psychological Association, Lawrence, Kansas, May 11, 1935. Written at the University of Missouri. I am indebted to Professor Harvey A. Carr of the University of Chicago for the suggestion of the specific title of this paper.

ponents which analysis of a given state might reveal or at the reactive possibilities at a particular time. It was assumed that at least some of the results of analysis had had an important past, but this was characteristically dismissed with a gesture or with a casual remark about dependence upon experience or about the existence of a genetic psychology.

The search for cross-sectional dimensions yielded and continues to yield a body of important facts. For long years these facts were psychology; anything else was application and technology. When genetic problems were attacked, the native components of behavior were the goal or, if growth and change of organization, whether native or acquired, were studied, the descriptions inclined toward successive cross-sections, while the principles of vertical (or longitudinal) organization were mainly disregarded.

The trend of psychology during the last decade or two has been away from cross-section and toward long-section, toward a study of the conditions under which progressive change of function occurs from before birth to senescence. A large body of data for an attempt at a vertical systematizing of psychology is already at hand and a system which could bind coherently together the sequence of cross-sectionally describable events, which could bring vertical order into their diversity, would be valuable. It is, indeed, doubtful if the data of psychology can be adequately systematized in any other way than vertically. The events called 'mental' never stand still; and cross-sectional description, while it can tell what changes, can never catch and picture their unceasing flow.

The Vertical Dimensions

The term 'vertical dimension' (or 'longitudinal dimension') means a variable which is present throughout practically all of the life-span and which, by virtue of its continuity must be present to some degree in any description of psychological events.

There are two major vertical dimensions of mind:[2] (1)

[2] 'Mind' is used here as a class term for those organic functions which are usually called psychological. This is not the place to attempt to state the criteria of psychological events. It suffices to say that no mind-body dichotomy is implied. Psycho-

maturation-degeneration and (2) learning-forgetting. Under the maturation-degeneration variable are included the entire organic and genetic aspects of the organism, together with their immediately correlated behavior phenomena, which mature and degenerate in relative independence of training factors. Under the learning-forgetting variable come the phenomena of stimulation and practice, the secondary elaborations of maturative development.[3] The two variables will be referred to hereafter, for the sake of brevity, as maturation and learning. The two are interrelated and in any given case one of them may be viewed as a condition of the other. Level of physiological maturation is, thus, a restrictive condition of learning, and whether learning is a condition of rate of maturation or not, it may certainly determine in many cases the specific effectiveness of a given level of maturation.[4] The two are, however, logically and operationally distinguishable for the purposes of both experiment and system.

Learning As The Major Vertical Dimension

This paper will deal only with learning, which is, for three reasons, of particular systematic[5] importance. (1) Maturation is powerless to produce without learning any but a few of the functions called mental. (2) Learning is the directly controllable variable in human subjects and is, therefore, of greater experimental importance. (3) Learning more intimately and specifically pervades the entire developmental span of life. The organic framework and setting within which

logical events are organic functions of a particular kind. Nothing more is needed in the present context than the rough working usage whereby psychologists mark off their field from other fields.

[3] *Cf.* C. P. Stone, Learning: I. The factor of maturation, Chap. 8 in 'A handbook of general experimental psychology' (Ed. by C. Murchison), Worcester: Clark University Press, 1934. See also C. P. Stone, Maturation and 'instinctive' functions, Chap. 3 in 'Comparative psychology' (Ed. by F. A. Moss), New York: Prentice-Hall, 1934.

[4] For a survey of the unlearned origins of behavior, see L. Carmichael, Origin and prenatal growth of behavior, Chap. 2 in 'A handbook of child psychology' (Sec. ed. rev., Ed. by C. Murchison), Worcester: Clark University Press, 1933.

[5] The word 'system,' as it is used here, means a set of internally coherent concepts by means of which data may be interpreted and ordered. It implies nothing at all about the so-called 'schools' of psychology.

learning takes place and by which its limits are determined may belong almost entirely to the maturation-degeneration dimension. It is the biologically more fundamental dimension. There can be no doubt, however, that the specific responses of the individual to specific stimuli, situations and the like are, aside from a relatively small number of native ones, primarily determined by learning; and it may be contended that the specific character of all non-reflexive acts as performed by an adult organism is learned.

The Problem of Definition

A discussion of learning as a major dimension of mind may seem to call at the outset for a formal definition of learning. I shall not, however, attempt such a definition. It would be useless to define the concept *learning* without developing in detail the evidence for the adequacy of the definition. To do this would take too long. There is, moreover, no disagreement among psychologists over the major aspects of the facts of learning. Serious disagreement enters only over borderline cases, such as hysteresis or sensory adaptation, and over theoretical interpretations of the facts.

I shall mean by *learning* the phenomena of the learning curves, of the distribution-of-practice experiments, of the immediate memory experiments, and of the host of others of similar kind arrived at and defined by well-known operations. Under the concept learning are included the facts of *retention*, since the two are inseparably intertwined, and the facts of *transfer*, since all learning involves transfer and only a sterile educational tradition sets it apart as an isolated category.

In lieu of a formal definition, a few restrictive statements may be made. It seems futile to define learning in terms of hypothetical factors such as neural changes, tensions, or the logical properties of total fields. These factors may conceivably aid interpretation, but they do not at present define learning. A definition must keep as clear as possible of theory and as close as possible to the facts, and to the conditions and operations by which they are obtained. A definition which can be given no specific meaning in terms of performable

operations is not a satisfactory experimentalist's definition. An operational definition is implied in what critical experimenters do and write. It is implied here in saying that by learning is meant the phenomena of the learning curves and of the learning experiments in general.

Much of what is to be said in this paper is an extrapolation of actually performed operations. The hypotheses stated lead to performable operations or to an extension of already standard ones and therein lies their claim to significance. The extrapolations are subject to experimental check; if they were not, they would be futile, or at best an exercise in phantasy. Experimental check implies operational analysis, and not far beyond the reach of operational analysis lie the quagmires of speculation, the half-world of demi-concepts in which the arm-chair theorist lives and laryngeates.

The Historical Place of the Concept of Learning

The place of the concept of learning in the historical development of psychology is, in spite of its present importance, relatively slight. Boring[6] has written a 'History of Experimental Psychology' without giving it much more than a passing mention, and even if we hold that Boring minimized its historical importance, we cannot say more than that it had become at the beginning of the 20th century an important special topic and that it remained little more than this until well into the 1920's. The other histories give it no more general importance than this, although most of them, including Boring, refer to the work of Ebbinghaus as epoch-making.[7]

The early systematists, writing of the phenomena of learning under the rubrics of association, habit, retentiveness and memory, gave a greater place to the general concept than

[6] E. G. Boring, A history of experimental psychology, New York: Century, 1929.

[7] Murphy evaluates Ebbinghaus' quantitative studies of memory and forgetting as "probably the greatest triumph of original genius in experimental psychology since Weber." See G. Murphy, An historical introduction to modern psychology, New York: Harcourt, Brace, 1929, p. 189. Langfeld, however, in reviewing this book (*Psychol. Bull.*, 1930, 27, 132–134), styles Murphy's evaluation "a bit strong," and there are probably many who would agree with Langfeld. The writer agrees with Murphy but does not believe that the systematic generality of the implications of Ebbinghaus' work was realized until comparatively recently.

did the experimentalists. They had at hand, however, relatively few experimental data upon a limited set of phenomena within the category of cognition, and although some recognized the generality of the concepts we now class under learning, they only partially developed the implications of these concepts. The earlier American systematists carried it little farther. To Titchener, as to most of his students of the earlier years, learning was beyond the confines of science. The functionalists and behaviorists brought it forward as a major concept, but to many of them it was still a special concept, not a general one.

In this country, as elsewhere, learning has until recently been thought of as belonging more to educational psychology than to general experimental and systematic psychology.[8] It has been thought to be valuable to the teacher, but for other fields and particularly for the general systematist it has been a secondary concept. Especially within the last 10 years, however, the perspective has been changing until we now find psychologists who assert that learning is the most fundamental of all psychological phenomena.[9] The systematically fundamental and pervasive character of learning remains, however, to be worked out explicitly.[10]

It has been traditional, of course, to refer many psychological phenomena to 'past experience.' This means that they are learned, but such references to learning have been casually made and explicit statements of their implications are

[8] For a review of some of the work which shows the importance of learning as a general concept, see J. A. McGeoch, Learning and retention of verbal materials, *Psychol. Bull.*, 1934, 31, 381–407.

[9] Bills, for example, writes: "Learning is the most fundamental psychological phenomenon, since it is the basis on which the entire super-structure of complex mental behavior is erected." See A. G. Bills, General experimental psychology, New York: Longmans, Green, 1934, p. 175.

[10] It has received implicit systematic exposition in H. A. Carr's 'Psychology,' New York: Longmans, Green, 1925, which is in effect a demonstration of the way in which learning pervades the other categories of psychology. Carr does not say that learning pervades other categories, but he makes it so clear to the reader that it does that the point can hardly be missed. Hollingworth's systematization of psychology in terms of the concept of redintegration amounts to a systematization in terms of the concept of learning regarded in a particular way. *Cf.* H. L. Hollingworth, Psychology, New York: Appleton, 1928.

lacking. In current textbooks and systematic discussions, moreover, learning is treated as if it were a special and compartmented set of events having little relation to perception, affection and the rest.

It will be well, therefore, to review briefly the manner in which learning pervades some of the major categories of psychology. What is to be said will appear obvious to some. Insofar as one can judge from current writing, however, it is by no means sufficiently obvious and acceptable to be taken for granted. Recent general textbooks and journal papers may be searched in vain for any systematic use of the concept of learning. An author may say that it is important but he does not use it. This failure to employ the concept as a fundamental one has been mirrored in experimental work. There has been some work which carries learning through other categories, but the amount which does this is relatively small. What is now to be said is, therefore, partly hypothesis but it is hypothesis which departs from the laboratory and which leads directly back to the laboratory.

Perception

We shall begin with perception. The textbook accounts are mainly cross-sectional descriptions of what perceiving is and what its immediate conditions are. It is a psychological truism, of course, that perceptions are a function of past experience, but this reference to experience can be given systematic meaning only if we can state the conditions under which experience has been effective. An attempt to do this leads at once to the concept of learning. It detracts in no way from the value of horizontal descriptions to point out that any given case of perceiving is primarily determined by prior learning and is, in turn, itself an instance of learning which acts as a determiner of later perceiving. Perceiving, that is, is a partial cross-sectional aspect of the continuum of learning.[11] It attains independent status for systematic analysis as a cross-section of this continuum.

[11] Guthrie's interpretation of perception as a result of conditioning is an attempt to account for it in terms of learning. See E. R. Guthrie, The psychology of learning, New York: Harpers, 1935, Chap. 16.

Perceiving offers a wealth of illustrations of transfer. Objects perceived seldom appear in exactly the same settings as the one in which they were learned and they are often met in very different ones. Prior perceptual learning transfers from the old situation to the new. To interpret perception in terms of learning and transfer is to do no violence to either major category. Perception remains as important as ever, but is given its place in a continuous systematic perspective.

The experimental work on space perception yields cogent specific evidence in support of the dominance of learning in perceiving. The work of Stratton,[12] Wooster,[13] and Ewert [14] on vision without inversion of the retinal image, of Young [15] on the pseudophonic transposition of the two ears, of Renshaw and others [16] on change in accuracy of tactual localization with practice are examples. The basic importance of the subject's response in the building up of reactions to space relations, which has long been recognized, is another bit of evidence which takes perception out of its one-time place as a purely receptive function and places it in relation to learning.[17]

The dominance of learning has been shown experimentally in many other instances of perceiving. Moore's [18] demonstration that perception of intervals as consonant or dissonant is a function of practice, and Farnsworth's [19] recent support of this

[12] G. M. Stratton, Some preliminary experiments in vision without inversion of the retinal image, PSYCHOL. REV.. 1896, 3, 611–617; Upright vision and the retinal image, PSYCHOL. REV., 1897, 4, 182–187; Vision without inversion of the retinal image, PSYCHOL. REV., 1897, 4, 341–360 and 463–481; and The spatial harmony of touch and sight, *Mind*, 1899, 8, 492–505.

[13] M. Wooster, Certain factors in the development of a new spatial coördination, *Psychol. Monog.*, 1923, 32, No. 146, pp. 96.

[14] P. H. Ewert, A study of the effect of inverted retinal stimulation upon spatially coördinated behavior, *Genet. Psychol. Monog.*, 1930, 7, 177–363.

[15] P. T. Young, Auditory localization with acoustical transposition of the ears, *J. Exper. Psychol.*, 1928, 11, 399–429.

[16] S. Renshaw, The errors of cutaneous localization and the effect of practice on the localizing movement in children and adults, *J. Genet. Psychol.*, 1930, 38, 223–238; S. Renshaw, R. J. Wherry, and J. C. Newlin, Cutaneous localization in congenitally blind versus seeing children and adults, *J. Genet. Psychol.*, 1930, 38, 239–248.

[17] Much of this evidence for the relations between learning and perception has been discussed by Joseph Peterson, Aspects of learning, PSYCHOL. REV., 1935, 42, 1–27.

[18] H. T. Moore, The genetic aspect of consonance and dissonance, *Psychol. Monog.*, 1914, 17, No. 73, pp. 68.

[19] P. R. Farnsworth, A study of the extent to which similar tonal elements are accepted by three different age groups, *Genet. Psychol. Monog.*, 1934, 15, 7–9.

conclusion, are cases in point. So also is the change with practice even in the relatively simple perceiving involved in psychophysical judgments, in the perception of the Müller-Lyer illusion, in the perception of time-intervals, in the perception of facial expression, and in many other perceptual functions.

The fact of learning in these functions has been often reported and is well known, but it has commonly been regarded as a special and relatively unimportant aspect of an activity which was itself independent of learning. It is suggested here that the learning reported is a case of continued learning in a function already somewhat advanced on its practice curve or the learning of a comparatively new function assisted by transfer from other already learned ones. It is not to be expected, however, that a learning curve will appear in all experiments upon perception. In some of them the conditions are such as largely to preclude learning; in others the functions studied have had an opportunity to reach a stable practice level before the beginning of the experiment.

Ideation and Reasoning

We arrive at a category of perception only by abstracting from the total complexity of events. When we abstract in different ways we use the categories of thinking or ideation and reasoning. I shall mean by an idea any aspect of experience or behavior which is playing a representative or symbolic function, and by reasoning the solution of problems primarily by means of ideas. It is usual in dealing with ideation to describe what ideas are and do and, perhaps, to refer them to experience but otherwise to leave them unconnected with the individual's past history. Reasoning is commonly described as a trial and error process, and is thereby related to learning, but by only a few writers is the relation explicitly asserted to be intimate.

The prevailing empirical view of the origin of the specific symbolic reference of ideas directly implies, however, that they are learned. Many of the aspects of experience and behavior which are the carriers of the reference are native, but the major point is that their specific symbolic function is learned,

as is the order and pattern in which they appear. To the extent that learning involves a substitution of one stimulus or response for another, and it can be held that this is a major characteristic of what our operations measure, learning is a matter of establishing symbolic function. In any event the category of ideation rests, except for cross-sectional description, upon learning.

Reasoning is so clearly the analogue of motor learning, except for the amount of movement and complexity of ideational reference involved, that its relation to learning needs little elaboration.[20] Only an over-rationalizing philosophical tradition, which looked to type of problem solved rather than to the antecedents of solution, could enthrone reason as 'pure' and set apart from learning. The experimental work on concept formation, generalization and problem solving by both children and adults has provided a wealth of fact in support of the continuity of ideation and reasoning with learning.[21] In these functions transfer is a potent factor and one which apparently becomes more powerful as the reference of the ideas employed becomes more abstract.

Motivation

It may be possible to consider perception, ideation and reasoning as hung upon the thread of learning. It is with phenomena of this sort that most of the experimental work on learning has been done. But what of motivation, affection, emotion and the others which have been thus far outside the current of this kind of experimental attack? It will be well to begin with motivation since it is a fundamental concept in the

[20] The relations between the rational and the motor methods of learning are excellently set forth in Carr's 'Psychology,' Chap. 9.

[21] The literature here is too voluminous to cite. A good introduction to the work on the formation of ideas, concepts and generalizations and on rational problem solution in children can be found in M. W. Curti's 'Child psychology,' New York: Longmans, 1931; and in Joseph Peterson, Learning in children, Chap. 10 in 'Handbook of child psychology' (Ed. by C. Murchison), Worcester: Clark Univ. Press, 1933. C. L. Hull's 'Quantitative aspects of the evolution of concepts: an experimental study,' *Psychol. Monog.*, 1920, 28, No. 123, pp. 85, stands as a good example of work on the learning of concepts. *Cf.*, also, N. R. F. Maier, Reasoning in humans. I. On direction, *J. Comp. Psychol.*, 1930, 10, 115–143, and II. The solution of a problem and its appearance in consciousness, *J. Comp. Psychol.*, 1931, 12, 181–194.

understanding of action and one of the basic conditions of learning.

That there are a few unlearned biological conditions which serve as motivating conditions and a few incentive objects which natively satisfy these motives there seems to be no doubt. Their number, however, is small and very early in life they begin to be overlaid by learned behavior, until they are soon so complexly woven in with learned organization as to be present only by implication. Hunger, sex, escape from pain and other apparently biological motives operate directly and as such much less often in adult behavior than one might at first suppose. The adult exhibits the class of behavior to which these motives lead, but he exhibits it often in response to other cues than the original ones and in other ways than the native ones. He is acting, that is, in response to learned cues and he works toward symbolic rewards or away from symbolic discomforts. He seeks food at verbal cues, works for such rewards as social approval variously expressed, for biologically remote luxuries, or for such verbal stimuli as 'Right' or 'Wrong.'

The fact that a large part of the motivating conditions of behavior early becomes learned or symbolic does not mean that biologically motivating conditions are or were ineffective. It does mean that, while the biological conditions serve as starting points and by changing along the maturation-degeneration continuum may change their initiating potency, they do not long remain the only or the chief motivating conditions. The motivation function is rapidly taken over by learning and the two sets of conditions continue to be interacting variables, with learning bulking large in the interaction.

The conditions which are called sets, attitudes, interests and intentions are closely related to and are often indistinguishable from motivating conditions. One of the chief differences is that they are more subtle and symbolic than biological motives, although in the learning of them the biological motives probably served as starting points. That they are effective determiners of behavior is well known; that they are also learned is highly probable and almost certain.

The learned character of a majority of the motives, sets, attitudes, interests and intents which give direction and definition to much of the remainder of experience and behavior vastly extends the pervasiveness of learning.[22] The dynamic, directional conditions of behavior are by no means wholly native; they are, in fact, in organisms beyond some very early age, largely learned. The specificity of their reference and direction almost certainly is.

Affection

The phenomena which belong to the category of affection have long been studied primarily by the procedures of cross-sectional analysis. The problem has been to find what affection is and its cross-sectional conditions at the moment, and there has been little attempt to discover the history of a given affective response. It is desirable, of course, to regard affection as something to be described and to disregard its antecedents. Such horizontal description is necessary in the case of every phenomenon, but it is not the whole of the story in any case.

On the side of fact there is no doubt that most objects are pleasant, unpleasant or indifferent as a result of each individual's training.[23] Experimental fact and common observation combine to demonstrate it. This means that the specificity of most of our affective reactions is learned but it tells nothing about the character of the affective response as such. We shall proceed to this.

In his *judgmental theory of affection*, published in 1925, Carr[24] states a conception of feeling tone which places it squarely in the category of judgment. Pleasantness and unpleasantness are judgments which we make concerning a stimulus object on the basis of our normal reactions to it. Pleasantness is the judgment on a stimulus which normally elicits a positive response and unpleasantness is the judgment

[22] See, for example, E. L. Thorndike, The psychology of wants, interests and attitudes, New York: Appleton-Century, 1935.

[23] For some of the literature on this point, see J. G. Beebe-Center, The psychology of pleasantness and unpleasantness, Esp. Chap. 7, New York: Van Nostrand, 1932.

[24] *Op. cit.*, Note 10.

on a stimulus which normally arouses a negative reaction. In most cases the overt response is not aroused but the judgment is made on its meaning. Now judgments may be considered as solutions of problems arrived at largely through transfer from earlier training in similar situations. They have often been erected into a mystery because the emphasis has been placed on solutions when the alternatives were highly symbolic and the antecedents unknown, but decision between cul-de-sac and true path in maze learning is as much a judgment as a decision between two models of the atom or upon two methods of international finance. The judgmental theory, therefore, asserts that pleasantness and unpleasantness are judgments made on the basis of responses, a great many of which have been built up during the individual's history. Originally the maturation dimension was primary in the initiation of their connections with objects, but its influence early became enormously complicated by training.

This theory has not received the attention it deserves. Preoccupation with cross-sectional analysis has led research in an opposite direction. Recently Peters [25] has restated the judgmental theory and has convincingly marshalled the evidence in support of it. If the theory can incorporate the results of direct experimental attack, it brings affection to a considerable extent within the continuum of learning. On any theory, however, the affective qualities of most stimuli are learned and probably those of all are modifiable by practice.

Emotion

The organic core of emotion belongs to the physiological dimension of maturation, and any systematic description of emotion, whether in terms of native behavior or of cross-section at a given time, must lean heavily upon physiology. The moment, however, that we examine the conditions which arouse emotions we shift to the dimension of learning. The stimuli which will, at birth or as a result of maturation alone, arouse emotions are few; the effective stimuli later in life are

[25] H. N. Peters, The judgmental theory of pleasantness and unpleasantness, Psychol. Rev., 1935, 42., 354-386.

many. It has been shown experimentally that emotional responses may be readily and permanently conditioned and non-laboratory observation amply corroborates the experimental results. Stimulus objects which have been connected by training with emotional responses are far more frequent inciters of emotion than the few native stimuli. Overt emotional response may, likewise, be altered radically or inhibited entirely by training, and it is a plausible hypothesis that even the implicit physiological responses may be correspondingly altered.

It is possible, also, that the perception or the meaning of the exciting situation may be a major differentiating criterion between emotions, insofar as the subject's description and labelling of the emotion and his overt response to the situation are concerned.

In these ways the learning dimension is present in and pervades emotion, although it is a less dominant factor in emotional response than it is in such categories as perception or reasoning.

Personality

The phenomena studied under the concept of personality offer another illustration of the pervasiveness of the learning dimension. Both behavior and its organization are built upon organic conditions, but we need not speculate here upon the extent to which glandular functions, bodily physique and other maturation factors are basic. We inquire, rather, upon what the specific social behavior and the organization thereof depends.

The observations made in child welfare clinics and elsewhere upon the development of personality traits in children, together with clinical and other practical work upon the modification and reëducation of such traits, combine to support the conclusion that at least a very great many traits are what they are *specifically* as a result of training and that the *organization* among them is largely a function of training factors.

In the field of abnormal behavior the idea has steadily been gaining ground that the functional abnormalities are products

of learning. They may result from earlier habits carried over to adult life, from specific learning of maladjustive responses, or from spread effects which do not suit the social situation to which they transfer.[26] The conditions which lead to abnormal behavior are much like those in the conventional learning problems where there is multiple response and final selection among responses. The individual is faced with practical problems which demand solution. He may, largely through transfer of training, perhaps, hit upon abnormal responses and may fixate them if they happen to satisfy his motivating conditions.

The specific normal traits of personality and their organization may, on the basis of the facts available, usefully be systematized by the concept of learning. So also may abnormal traits and disorganization where there is no organic pathology to serve as a major condition. Even when there is organic pathology, as in senescence or in paresis, the disorganization of learned behavior offers important problems, although the disorganizing conditions are not the usual ones of ordinary forgetting.

Volition and Physiological Functions

There is not time to inquire the ways in which learning pervades all of the categories psychologists use. Two others may be mentioned briefly. What has already been said about motivation, set and attitude implies that learning is a major condition of voluntary action. Hudgins'[27] recent conditioning of pupillary constriction to a verbal stimulus and even to a subvocal whispering of a command suggests one way toward an understanding of voluntary responses as learned.

The success with which a number of physiological reactions, such as pupillary responses, vasomotor reactions, galvanic skin responses, gastric secretions, salivation, kidney,

[26] For a statement of this general point of view, see L. S. Hollingworth, Chap. 20 in 'The problem of mental disorder' (Ed. by M. Bentley and E. V. Cowdry), New York: McGraw-Hill, 1934.

[27] C. V. Hudgins, Conditioning and the voluntary control of the pupillary light reflex, *J. Gen. Psychol.*, 1933, 8, 3–51. *Cf.* L. C. Steckle and S. Renshaw, An investigation of the conditioned iridic reflex, *J. Gen. Psychol.*, 1934, 11, 3–23; and C. V. Hudgins, Steckle and Renshaw on the conditioned iridic reflex: a discussion, *J. Gen. Psychol.*, 1935, 12, 208–214.

bowel and bladder functions, and possibly even immunity reactions,[28] can be conditioned demonstrates that many of the physiological processes of the organism are subject to learned control. So, also, do such changes with practice as occur in nystagmus, chronaxie, and temperature regulation. Results of this sort point to an intimate connection of physiological and psychological events in ways not hitherto regarded as possible or important.

Learning in Extra-Laboratory Settings

The discussion thus far has dealt with the pervasiveness of learning in some of the major categories of technical systematizing. Learning is not, however, confined to the laboratory settings in which the technical data of these categories are worked out nor to any situation of formal learning. The tradition that learning belongs primarily to educational psychology, with the concentration of that field upon schoolroom learning, has led to a widespread assumption that the learning with which psychology deals is mainly the formal learning of the schools or of school-like situations.

On the contrary, the learning we study in the laboratory is representative, insofar as any set of events under controlled conditions can be representative of events under non-controlled and natural conditions, of much more than laboratory learning or any formal learning. The course of life during every waking hour and during some, at least, of the hours spent in sleep is the course of learning. Every perception is both determined by past learning and is itself a case of learning. So, also, is every idea, every instance of reasoning, every voluntary act, everything which happens to alter or add to experience and behavior after the paradigm of the operations which define learning.

Implications for Experiment

But, you may ask, if we grant all this, what difference does it make? The answer is that it makes a difference both to

[28] For a reference to much of this work, see C. L. Hull, Learning: II. The factor of the conditioned reflex, Chap. 9 in 'A handbook of general experimental psychology' (Ed. by C. Murchison), Worcester: Clark Univ. Press, 1934.

experimental work and to the systematic organization of experimental fact. We shall review first some of the implications which the pervasiveness of learning as a vertical dimension has for experimental work. These implications are the most important aspect of what has been said.

First of all, the problems of learning and retention gain increased significance and it becomes more important than ever to obtain an extensive and coherent set of learning data. This is especially true of the problem of the basic conditions. We know something about the fundamental conditions under which learning occurs; we need to know much more. There is a large number of problems under the general rubrics of motivation and effect, time variables and contiguity, frequency, remote association, transfer, associative spread and equivalent stimuli, to mention a few fundamental areas, the solution of which would be of great value. Since any learned response at any given time represents a residue after forgetting has been cancelled out, the primary conditions of forgetting are equally fundamental problems. The areas implied by interference effects from interpolated learning, by interference effects of other kinds, both inter-serial and intra-serial, and by changed stimulation offer a particularly fruitful set of experimental problems.

Whether one grants or not that the concepts of learning are defined by the measuring operations, the problems of methodology immediately become focal. We need a more standard, detailed and consistently used set of measuring operations. There is too much striving after originality of method which ends in trivial deviations and which yields results impossible of systematization with other results on account of the lack of uniformity in the operations. We need to learn, for example, that a standard use of a good method for the solution of fundamental problems is far more important than the bright invention of a methodological variation, often for the solution of a minor problem or of none at all.

The experimental problems of learning are much wider than they have often been held to be, and for the solution of these wider problems we need an extension of laboratory

methodology, but an extension which builds upon standard operations. These problems go far beyond the learning of the simple verbal materials, acts of skill and rational problems of the traditional learning experiments. There is much more to be discovered by work with these materials, but their use has limited the problems we have worked upon.

A great deal of the learning of daily life, the learning which enters intimately and directly into the structure of mental organization, is of the kind usually called 'incidental.' The relation between it, where there is no obvious problem and where frequency is low, and the usual forms of laboratory learning has not been experimentally worked out. The 'Aussage' experiments and those on incidental memory have made a beginning, but the more fundamental problems have been little more than touched upon.

The question of the relations between incidental and intentional learning suggests at once that of the relation between immediate memory and more permanent retention. There is at present a gap between the phenomena of the memory span and of learning by repeated trials. Recent work in neurophysiology has suggested that there may be a physiological basis for a distinction between them. However this may be, behavioral studies designed to explicate their relations are needed. Probably most of the continuous stream of perceiving, ideation, and other horizontally described mental events which enter into the complex texture of organization are cases of incidental learning, and an interpretation of these cross-sectional events as aspects of the vertical dimension of learning requires that the inter-relations of incidental and intentional learning, immediate and more permanent memory, be worked out.

The categories usually studied in cross-section offer an almost unexplored field for study by the operations of the learning experiment. A few representatives of the numerous possibilities will be reviewed. The application of what is known about the learning of other things to the learning of specific perceivings and an isolation of the influence of one learned perception upon the perception of other objects, that

is, transfer in perceiving, would illuminate both learning and perception. It could, also, throw light upon the ways in which the incidental learning of perception enters into mental organization. The special fields of space and time perception offer, likewise, a host of experimental problems which are directly attackable by the operations of learning.

It is steadily becoming clearer that the special problems of psychophysics are continuous with those of learning. Psychophysical judgments depend upon immediate memory and are, at the same time, subject to the same kinds of practice and interference effects which we find in learning by repeated trials. Psychophysics, as a phase of learning, provides problems which could be attacked by the more usual operations of the learning experiment to the profit of psychophysics, and it, in turn, offers methods which could be used widely in a study of other problems of learning.

Experimental studies of the acquisition and transfer value of symbolic function at any age, but particularly in young children, would help to remove the vagueness which envelops the concept of representative function. The facts thus gained would reach beyond this special topic into the other categories of psychology and, in some measure, beyond psychology into all fields of which ideas are the daily tool. Experimental analysis of reasoning, which has been fruitfully begun but which has been carried forward slowly, should be equally valuable. We know the rough outlines of the function reasoning as a psychological phenomenon, but there are many gaps. Partly because of earlier concern with horizontal analysis, we know little of the specific conditions which determine it. These conditions can be studied most fruitfully by the operations of the learning experiment.

The acquisitional overlay of motives and the establishment and functioning of symbolic motives and symbolic incentives can be attacked experimentally and the data thus obtained might go far to lift the concept of motivation in human subjects from the half-world in which it still often wanders to the clarity of quantitative statement. The learning of attitudes, intentions, interests and sets is, likewise, a fertile ground for

research. On the side of learning there is the question whether attitudes, interests and motives are learned under conditions similar to those which determine the learning of the more conventional materials and whether their learning can be brought within a similar conceptual formulation. On the side of a description of these phenomena and of their function, a knowledge of their development is almost a necessity. Thorndike's recent book, 'The Psychology of Wants, Interests and Attitudes,' [29] makes an important attack on some of these problems, and the work on change in aesthetic appreciation and in social attitudes with practice yields valuable data. The specific methodology here is difficult, but it has been shown that experimental work is possible and that the results pay for the work.

There is much to be done, likewise, in the learning of affective and emotional responses. Many of them seem to be learned more quickly and retained longer than the ordinary verbal materials and acts of skill. This and many other aspects of affection set problems to the psychology of learning as such, as well as to the category of affectivity. The judgmental hypothesis is, likewise, amenable to experimental test and should be tested. The connection of new stimuli to emotional responses and the reorganization and control of emotional responses constitute another set of significant problems. The data obtained from a study of these problems by the methods of the learning experiment might resolve some of the theoretical difficulties within these special cross-sectional fields. They might, for example, make clearer the differentia of emotions, the development of emotional expression, and the control of emotion for mental hygiene.

A systematic attack upon the learning of specific personality traits is needed for the purposes of both normal and abnormal psychology. It would throw light upon the problem of the specificity and generality of traits and upon the organization of social behavior. Analysis by the operations of learning might, also, permit personality testing to become more precise and less an amorphous but pious hope, and it might permit a more systematic definition of the concept of

[29] See Note 22.

personality. Repetition and extension of Hudgins' work, together with other experiments on the symbolic control of action, should, likewise, have far reaching theoretical implications for an understanding of voluntary action.

An extension of the work on the conditioning of physiological processes and of learning in the realm of intraorganic events would be of value. Such work is significant not only for the facts it yields but for what it shows of the interpenetration of physiological and psychological events and for what it may do in aiding a more satisfactory formulation of the classical mind-body problem in terms of an organic unity in which the events of the two classically separated orders can be specifically seen as parts of a continuous series.

These general fields can be attacked, of course, only in terms of specific problems and the number of such problems which each offers is great. Their experimental solution would both tell us much about learning and make possible a more consistent and inclusive formulation of other problems. It would be futile, however, to speculate upon results. The thing that is emphasized here is that the psychology of learning is much wider than it is sometimes thought to be and that many of the other categories of psychology offer problems which lie within the field of learning.

Some Systematic Implications

The assertion that learning is a variable or a vertical dimension which pervades most of the other phenomena of psychology makes a systematic as well as an experimental difference. Learning is not merely an individual category to be set off, as the textbooks do, in a chapter coordinate only with the data of cross-sectional description. It is, as a phenomenon, primarily a longitudinal set of events and, by virtue of the fact that it goes on continuously as a pervasive aspect of other phenomena, it constitutes one of the two chief developmental dimensions along which psychological phenomena naturally are organized. It may be suggested, therefore, that in an attempt to formulate the data of psychology under a coherent and unitary set of concepts the dimension of learning offers one of the most fundamental general concepts. It is

indispensable to a genetic psychology; it is also indispensable to a systematic formulation of the facts at any level of development where the concern is with more than cross-sectional description alone.

Some General Considerations

There are certain things which should now be said in order to avoid misconceptions. It has not been implied, first of all, that the methods of the learning experiments are the only ones which can be used upon these problems. The concepts and methods of learning are not panaceas, nostrums, or formulas put forward to save psychology. The experimental problems which have been suggested are extensions of work already done. Their attack involves no scrapping of old work or avowal of new faiths. It means only a continued development from within in directions in which we are already moving.

It is not implied, moreover, that learning is the only concept of psychology or that a new system or school should be founded around it. We need systematic, consecutive, operationally grounded work from within, however, if we are to construct a *psychology*, not schools of psychology. We know enough already about learning and its relations to other categories to permit us to see that it is a fundamental dimension. In this paper an attempt has been made only to explicate these relations and their general systematic significance.

No doubt should be left about what is meant by the reference of perception and other categories to learning. Nothing is explained by such a reference alone. Learning is not an agent; it does nothing. A phenomenon is not explained by saying that it is learned. The statement that it is learned names the class to which it belongs and implies the conditions which may, therefore, be expected to determine it and the operations by which it may be measured. The naming sets the stage for explanation, but it does nothing more.

Some care has been taken, also, to avoid commitment to any particular theoretical view of learning itself. The fact of learning stands on its own feet and the interpretations given here are equally valid on any theory of learning which holds

that it is a function of its conditions and that it may be operationally measured.[30]

Summary

We may now proceed to a summary. Psychological phenomena may be studied either in cross-section or in long-section. There are, from the standpoint of the long-section, two major vertical dimensions, maturation-degeneration and learning. Learning is, in the light of recent experimental work, a much wider concept than the traditional one of educational psychology or even of the laboratory. Examples have been cited of the ways in which learning pervades the other phenomena of psychology, analysis of which has been primarily cross-sectional.

The modes of cross-sectional description are, of course, not isolated from learning. Perception, ideation and the rest, as horizontally studied, are moments in the flow of learning. The specificity of each is a function of prior learning and the occurrence of each is a condition of present and of later learning. Each is a brief cross-section in a flow of events determined by the endogenous conditions of the maturation-degeneration continuum and by learning-forgetting. Each rests upon its own past history and points forward as a potential determiner of future history.

This formulation of learning as a major vertical dimension suggests a large number of experimental problems and implies a widening of the field of the psychology of learning. This is, perhaps, its chief merit. It implies, also, that the variable of learning provides a major dimension in any longitudinal systematic organization of psychological fact.

[30] It is unnecessary to raise the nurture-nature issue in this context. There are both native and learned components present, at least by implication, in every mental activity. There is no necessity to say anything here about the relative weights of the two variables. Each is important and the relative weights are matters for experimental determination. For the purpose of the present paper it is sufficient to say that the human being is born with a particular genetic constitution. The quality of his organism in its various aspects sets limits to function and determines capacity. The native behavior and other native conditions which the organism brings with it are the necessary starting points of learning. These and the other aspects of the organism may mature and degenerate. Learning can go on only within organic limits, but within these limits the specific behavior which appears, together with its organization, is learned.

THE POSTULATES OF PSYCHOLOGY[1]

BY JOHN E. WINTER

West Virginia University

A few years ago an eminent Italian scientist, Federigo Enriques, wrote a book in which he took occasion to emphasize the imperfection, the relativity, and the tentative and inadequate character of all scientific and theoretical construction. In his opinion, however, there was adequate cause for this apparently unsatisfactory situation of the sciences. To quote briefly and freely: "A double fatality hangs over one who has consecrated his days to science. First, if he would contribute to the advancement of science he must prepare himself by a patient study of thousands of details which constitute its technique; he must learn the results obtained by numberless laborers whose researches tend toward the same aim. He must master their conceptions and subject them to a new criticism. This work so engrosses the attention of the investigator that he has little time left for the second part of his task, namely, casting a glance over the branches of science which are developing beside him. . . . Most investigators, if they are not rightly directed, shut themselves up in a narrow circle and fall into a blind empiricism. Others lose themselves in the region of confused generalities. . . . To avoid the possible tragic results of such a procedure it is necessary that those who are engaged in the development of any special branch of study should be conscious of the unity of the aims of science" (14).

There is undoubtedly this tendency for each investigator to shut himself up within his own field of investigation and to assume a superficial and sometimes contemptuous attitude toward the facts and problems of other fields of investigation. Psychology is among those branches of science that have been

[1] Presidential address delivered before the Southern Society for Philosophy and Psychology, Nashville, Tennessee, April 19, 1935.

considered at times either superficially or with suspicion and contempt. In addition to this attitude from critics outside the field of psychology, notably the physical scientists, psychology has had to contend with the contempt heaped upon it by some of its own erstwhile devotees.

This skeptical attitude toward psychology dates back to the first half of the preceding century when psychology was closely affiliated with philosophy and when the latter received such a scathing denunciation at the hands of Comte (**27**), (**30**). Later on the physical scientists voiced their objection to the trend of psychology. Today, however, if we make a survey of all the sciences, including psychology, from the standpoint of their hypotheses, their methodology and conclusions, we are struck with an interesting and significant trend among the physical sciences in contrast with psychology. It is this contrast to which I wish to call attention.

Formerly the tendency on the part of the physical sciences was to berate psychology on the ground of its inexactitude, lack of proper basis for predictability, and its tendency toward mysticism; and the hope and the ambition of psychology was to develop toward the apparent perfection enjoyed by the physical sciences. Today the situation as far as the physical scientists are concerned is radically altered, if not completely reversed. The physical sciences are now beginning to admit the relative character of their postulates, the non-predictability of some of their principles and the reduction of some of their data to the level of mere probability, thus placing themselves in the situation formerly accorded to psychology.

To cap the climax of this change in the mutual relations between the physical sciences and psychology we have what Williams calls the strangest anomaly of recent science, namely, that "while an influential number of physicists, once supposed to be students of physical nature, are suggesting that only conscious experience exists, an equally influential number of psychologists, once supposed to be students of consciousness, have suggested that only physical nature exists" (**32**). This is indeed no occasion for psychology to be proud or self-satisfied; nevertheless it will be difficult for it not to experience

a certain degree of satisfaction in the discovery that after all the chasm between the physical sciences and psychology is not as wide or as serious as it was formerly purported to be.

The Postulates of Physics

The task of the scientist is complex and penetrating, yet it is more modest and restricted than that of the philosopher, for while the latter deals with the eternal and ever tantalizing problem of the 'why' of the universe, the former limits his investigation to the 'how' of the universe. The scientist seeks to reduce observed facts to a system based upon certain fundamental principles. When this system is once established, it offers a background for the classification and explanation of new phenomena. The principles are based upon facts, are chosen to suit the facts, and are absolutely necessary to the construction of a science. Andrade (1) compares the methodology of science to that of a game. In any game, he says, there must be rules. "If you ask why in baseball a batsman is out if he is caught off base, the only answer is that that is the way the game is played. The question is a meaningless one if by it the questioner wants an answer in terms of something other than the rules of the game. So in physics, mass and energy in its various forms, such as electricity and heat, are fundamental concepts. If now, for example, one asks what electricity is, and he expects an answer in terms other than electricity, the question is meaningless to the physicist. It is absurd to think of it, for example, as a liquid when in reality the properties of liquids are explained in terms of electricity. The physicist can tell what electricity does, but not what it is. If one asks whether what is known about electricity can be explained in terms of something more fundamental, the answer is no! Whether these principles are in the last analysis true or ultimate is not a matter on which the man of science feels called upon to decide. If their consequences agree with nature they are at least provisionally true, and an advance in science may be said to occur when a wider range of phenomena has been brought within the scope of one general principle, and it follows that a scientific theory

may be abandoned when it has proved itself inadequate to its task."

The application of this latter principle has not only safeguarded the validity of science, but it has yielded surprising results, so much so that it has led not only to the overthrow of worn-out or inadequate postulates, but to the disconcerting conclusion that what was formerly considered predictable within the narrow confines of carefully sifted principles has now in some important instances been proven to be non-predictable, and only probable or possible.

Andrade uses the atomic theory of the universe in its development as a good example of all the above phases in scientific theory. He says that some forty years ago atoms were generally considered hard, unbreakable entities, each element possessing a definite type of atom, fundamentally different from that of any other element; that this hypothesis was sufficient to explain the properties of gases, for by supposing that atoms of this kind possessed certain motions which obey the laws of mechanics, results could be deduced mathematically which agree with the properties of gases as observed in the laboratory; that when the electron was discovered, it was thought that different types of atoms might be built up of electrons, but that the further discovery of radioactivity and the implication that certain atoms possessed within themselves a store of energy, made it necessary for the physicist either to admit this internal energy or to deny the postulate of the conservation of energy. The outcome of these discoveries, he says, led to the retaining of the postulate of conservation of energy, but to a denial of the unbreakability of the atom. In place of the former concept of the atom as the unbreakable unit of mass we now have mass unrelated to matter, reduced in size and functioning as the nucleus or as an integral part of an electrical charge.

From the above discussion of Andrade it follows that mass and matter are no longer identical terms. Moreover, in place of the one fundamental concept of electricity we now have protons, electrons, neutrons and photons, as separable forms of energy. From this one circumstance alone, it would seem,

we are justified in the observation that as physics advances in discovery it not only eliminates old, erroneous and useless concepts, but it is just as ready to add new fundamental concepts to account adequately for newly discovered phenomena.

But the recent discoveries concerning the atom have yielded another astounding fact, which in turn requires not, as in the previous cases, the abandonment of one concept for another, but a revision of one of the fundamental postulates of all science, namely, the principle of predictability. The quantum theory concludes that one cannot exactly predict the behavior of a single electron (2). One may determine its speed and its position separately, but speed and position cannot be measured together. Hence, one does not know where the electron may go the next moment. Here, then, as far as the present knowledge of the physicist is concerned, predictability must yield to probability.

If one challenges this assertion on the ground that the quantum theory applies merely to individual atoms, and that the ordinary laws of physics, engineering and chemistry are laws governing the behavior of large groups of molecules or atoms, Andrade retorts that if the physicist wishes to maintain complete consistency, as of course the true physicist does, he will admit that this new found character of the atom introduces probability in place of certainty in his experimental results.

An extreme case will illustrate this point. Can the physicist predict how the molecules will move in a solid substance like the edge of a razor blade? What chance is there that they will move in such a way as to cause the edge of the blade to become sharper? Not one chance in millions of millions, yet we are here in the realm of probability, and cannot make absolute predictions. Practically, we may say it will never happen; theoretically, however, it may happen.

Andrade uses coin tossing as an illustration of another point. What is the probability of getting 60 heads or tails out of a hundred? He says the chances are about 1 to 33. In 10 throws the chances of getting 6 heads are about 1 to 2. But in these cases we cannot predict what any individual

throw will yield. This statistical way of regarding physical problems, this conception that what we see is an average effect, and that we must be very careful before we attribute to individual molecules properties and laws discovered in matter in bulk, is assuming a large part in modern physics (**3**). At one time universal validity was attributed to any law which had a success with a wide range of experiments; today, according to Andrade, the utmost care is taken to distinguish between facts of observation and speculations introduced to explain them. Experiments after all give us statistics, and statistics, unless they are complete, can at best yield only probabilities.

It is thus no longer a valid procedure in science to consider a certain statement as a law on the ground that no exception has been found in it. Either a law is not what it purports to be, namely, a statement of an absolute and fixed relationship between certain phenomena, but is a relative statement based on a possible or imperfect induction; or we must admit the possibility that many so-called laws are not genuinely established laws. They are only statements of probabilities.

From these general observations we conclude then that the physicist unblushingly adds to his fundamental concepts as the occasion demands. He preserves the principle of the law of parsimony, but does not yield to the temptation to reduce his world to one thing in the interest of a fictitious philosophical principle of absolute unity. Neither does he abandon any concept until he has found a more adequate one which will embody all the facts that have been explained, however inadequately, by the now abandoned concept.

Mathematics

Let us turn next to the field of mathematics. Here, if anywhere, we are in the realm of absolute certainty and predictability, at least so we have been led to believe. Euclidean geometry, for example, was based upon certain indefinable axioms, such as the point, the straight line, and the plane (**22**), (**13**), and the statements of these axioms were assumed to be incontrovertible. They are intuitively appre-

hended, and any attempt to establish their truthfulness was entirely gratuitous. But during the last century there has developed a non-Euclidean geometry, in the light of which geometrical concepts, in so far as they refer to empirical reality, cannot at all lay claim to that rigid certainty which used to be regarded as one of the strongest arguments in favor of their a priori character (**15**).

The development of this new geometry unearthed various problems. To begin with, there is the fundamental problem as to whether space is a real object or a set of abstract relations, or a set of relations that have a reality all their own. Added to that is the allied problem as to whether spatial relations, if they are to be considered real, have any reality independent of bodies, or are an integral part of physics (**16**). If they are derived from physics, the exactitude of geometry is thereby impaired. In the measurement of an isosceles triangle, for example, the lines and angles approach perfection, but do not reach it. There is always some error in calculation. Hypothetical equalities are in fact inequalities, and the erstwhile theoretically perfect does not exist in actuality. This is the conception and conclusion of the realist in spatial theory. From this standpoint the earlier geometry is only a symbol of an empirical real that never exists.

But the reinterpretation of space had further implications. By casting a doubt on the axiomatic character of parallel lines resulting from the construction of two lines, both of which are in the same plane and at right angles to a third line in the same plane, and assuming that no parallels exist, non-Euclidean geometry claimed that the sum of the angles of a triangle is greater than two right angles (**23**). Here, then, the term triangle has a relative value only, and the sum of the interior angles is a variable, depending upon the particular group of axioms employed.

Hilbert, choosing his own set of axioms, comes to the conclusion that the true straight line, far from being what Euclid supposedly conceived it to be, is a continuous curve, possessing no double points, and having with any other straight line at most one point in common (**24**). In fact, according to some

authorities it is not impossible to construct many diverse systems of geometry based upon various theories of the physical constitution of space.

It would seem that these varying interpretations and varying groups of postulates point to the arbitrary character of this phase of mathematics, and detract from the certainty with which mathematics was formerly regarded. The so-called indefinables and absolutes of geometry are in reality variable and relative.

Biology

In the field of biology progress has been more gradual than in the other physical sciences, due in part to the great complexity of organic structure and the consequent multiplicity of detail that must be considered. The first step in the modern application of scientific method consisted in the study of the structure of organisms and in the application of chemistry and physics to this study. This procedure yielded, among other things, the important discovery that there was no essential difference in the constitution of organic and inorganic matter, and that physiological processes are often physico-chemical phenomena (**17**). On the basis that urea, an organic compound, had been synthesized it was assumed that all constituents of the body could be synthesized, and this conclusion had a profound effect upon the underlying postulates of biology by silencing for the time being the vitalistic theory of biology which maintains that there is something more in the phenomena of life than chemical and physical processes, and establishing more firmly the mechanistic theory (**6**). Weismann considered embryology a mechanistic process. Development consisted merely in the growing of a small machine, the embryo (**28**). Each cell by segmentation represented one particular fraction of the individual, and was predetermined to form that particular part. Certain biologists even produced chemical substances closely resembling cell structures which they hoped might become as genuine as natural living cells (**18**). Most important of all, pieces of living tissue cut off from some part of a living organism and immersed in a

suitable liquid have been kept alive for an indefinite period and have even been known to develop into diverse structures.

However, in spite of much brilliant work which tended to substantiate the mechanistic view of biology, physics and chemistry have been found inadequate, in the opinion of Enriques, to account for the peculiar nature and function of cells. They offer no explanation for the processes of fertilization, heredity and the determination of sex.

In refutation of the theory that each cell represented some specific fraction of the individual, Driesch (**8**), (**9**), by separating segmented cells obtained a whole individual from each segmented part instead of a part of an individual from each. Furthermore, if some of the cells of a living embryo were cut away, the remainder of the cells still formed a normal individual, but of smaller size. Also, when two eggs were forced together they combined to form, not two individuals, but a single organism, each of the two eggs forming half of it.

The chemical substances so ingeniously developed were after all not alive, and at the best represented remarkable analogies to the true living cell, and the tissue that was kept alive indefinitely after its severance from the living organism was already alive at the beginning of the experiment. In no case has life been produced from inanimate matter, and thus the most fundamental problem in biology has been left untouched and unexplained by mechanistic biology. The failure, or at least the inadequacy, of the physico-chemical approach to the solution of the biological problem has on the one hand opened the way for the present theories of heredity, and on the other has reopened the argument for a vitalistic conception of biology, such as that of Haldane (**20**).

Modern biology thus presents a wonderful galaxy of brilliant experiments and researches, with the astounding result that for almost every hypothesis, such as the theory that acquired characteristics can be inherited, the theory of natural selection, the theory that only characters affecting the reproductive organs can be inherited, the Mendelian theory, the mutation theory, the gene theory, the theory of emergent evolution, and the theory of vital force, a body of fact has been discovered to substantiate it—even those that are apparently

diametrically opposed to each other. But the diversity of results is ample proof that biology has not yet reached the status of a positive science in the strict interpretation of that term. In fact, one of the earlier modern hypotheses, namely, Darwin's theory of 'Natural Selection,' marked a step away from what was up to that time thought to be an immutable law of heredity and toward one that was changeable and far less certain in its predictability. The vast array of statistics that have been gathered in the study of each of these hypotheses has not advanced these hypotheses beyond the stage of probability. Even though an hypothesis may hold consistently within a group of phenomena, as, for example, Mendel's law when applied to the color of flowers bred from pure stock of different colors, yet absolute prediction is not possible in each individual member of the group. It would be a mistake, therefore, to attribute to these views a positive value that they do not as yet possess (**19**), (**7**), (**29**).

As for the most fundamental problem in biology, the phenomenon of life, the lack of an adequate physico-chemical explanation opens the way for the introduction of a concept that is qualitatively different from those advanced by either physics or chemistry. The failure of the majority of biologists to determine upon a postulate that will account for the phenomena that have hitherto defied explanation from the standpoint of physics and chemistry, is in my estimation a weakness of modern biology.

Psychology

Psychology accepts at least one postulate without dispute, namely, the existence of a living organism. This much at least it has in common with biology. Unlike biology, it has not felt the need of seeking to account for the origin of life, whether through the interaction of physico-chemical processes or through the assumption of an unknown, mysterious energy. It accepts the postulate of life unconditionally and without dispute.

When we come to that peculiar manifestation of life known as consciousness or mind, however, there is a distinct cleavage of opinion as to whether this form of experience shall be con-

sidered a postulate. In the attempted solution of this problem we must sooner or later subject our findings to the scrutiny of the law of parsimony. Do we actually need any further postulate than that of the living organism? As we discuss this problem it may be well to keep in mind that the modern physicist has found it necessary not only to eliminate one erstwhile postulate, the atom, but to add two or three others to take the place of the one that was dropped. Whatever the number of postulates may be that we shall find it necessary to assume in psychology, we shall at least have to assume a number large enough to account for all of the facts of mental experience or behavior.

We are thus faced with the problem of consciousness as a manifestation of mental behavior. Is it a necessary postulate or can we account for the totality of our experience without it? If we assume consciousness as a postulate, a further problem presents itself. Can consciousness take a legitimate place among scientific concepts, or do we, by assuming it as a postulate, compromise or nullify the right of psychology to be called scientific?

In the attempt to answer these questions two distinct views have appeared: one of these seeks to preserve consciousness as a scientific concept on the ground that it can be analyzed into purely physical elements or phases of experience; another, assuming consciousness to imply a non-physical experience, rejects it as a scientific postulate.

Neither of these views represents, in our estimation, an adequate solution of the problem of consciousness. Lashley (**26**), for example, says that consciousness is literally a bodily event—a reaction to the bodily mechanism—and that a physiological account of behavior will be a complete and adequate account of the phenomena of consciousness. Introspection for him is a valid psychological method, but what introspection yields is nothing but a series of sensory processes, visual, auditory, kinæsthetic, etc. For Lashley, "I am conscious" means "such and such physiological processes are going on within me." He makes a determined, and even heroic, but futile attempt to sidestep the unique character of mental as distinct from physical phenomena.

Boring (5), in his physical dimensions of consciousness, includes such characters as quality, intensity, extensity, and protensity, seeking in each case the physiological equivalent of the process under consideration. Extensity, for example, may require a three-dimensional projection in the brain. Yet here again, while these characteristics of consciousness are legitimate, a description of consciousness in terms of such characters as are common to physical and mental phenomena does not account adequately for the qualitative difference between the stimulations of nerves in various ways and the mental experience that acquaints us with these various physical experiences.

Watson (31), on the other hand, refuses to admit consciousness as a scientific concept, although he did at first state he was willing to accept and use the term in the same way that the physicist or the biologist might use it. But in excluding consciousness and making psychology a mere branch of natural science, he eliminates from this field a vast array of phenomena that have hitherto formed the very backbone of psychology. The chief criticism of Watson's theory is that he presses the law of parsimony at the sacrifice of fact and truth. More recently Bentley (4) seems to have radically departed from his earlier utterances on the problem of consciousness, for he now affirms that the body is the complete existential structure of the organism and that thought is a function of the body, much as digestion is a function of the stomach and allied organs.

These modern attempts to reduce or eliminate the unique character of consciousness are, of course, not new. Long ago Comte also tried to reduce the study of psychology to a mere chapter of physiology, but the attempt to reduce psychology to physiology meets with a difficulty similar to that which we found to stand in the way of reducing biology to a combination of physics and chemistry. It fails to take cognizance of certain vital facts.

The foundation for this attempt is nothing more than a hope—a hope that its methodology is adequate—and a will to persist in this hope despite the fact that up to the present it has not yielded the expected results. In this attitude the be-

haviorist is less logical than the physicist, for the latter assumes an hypothesis for the explanation of every unknown, and does not abandon it until he has positive evidence of a more adequate hypothesis, one that does not ignore a part of the facts but that includes all pertinent facts. The electron theory, for example, does not ignore any of the facts covered by the atomic theory. The way is thus open in psychology for the introduction of such a postulate as shall adequately account for the unique character of mental phenomena.

Behaviorism has made another demand on psychology in its effort to make it scientific, namely, that its principles and conclusions shall possess the character of predictability. The application of this criterion has led to the further elimination of facts and principles that were considered perfectly legitimate by the non-behaviorists. This latter demand (of predictability) raises the question of whether upon this basis there is any possibility of constructing a science of psychology. In the answer to this question it will be fruitful to consider the modern trend in the physical sciences, in the light of which we may better evaluate the behavioristic demand on psychology.

Absolute predictability demands among other things exact measurement, and measurement is, of course, common to all the sciences. But while measurement appears to be a purely objective process and therefore capable of absolute exactitude, the actual fact is that errors in measurement are inevitable in all the sciences through illusions of sense, through changes in the physiological condition of the sense organs in each individual, and through individual differences in groups of individuals. Again, in any case of measurement, aside from possible defects in the measuring instrument, which may be considered as purely objective, variations are introduced through the various methods of handling the instrument, the psychological condition of the experimenter and changing conditions of the environment.

As a result of the inevitable variations in statistical results accruing from variable measurements, the experimenter in each of the physical sciences resorts to the 'mean' as a measure of approximation to accuracy. Absolute accuracy in physical

science is a goal seldom if ever attained. So long as the physical sciences still labor under the handicap of subjective factors it is, in our opinion, expecting too much of psychology that it yield hypotheses and results that are free from all subjective taint.

In addition to exact measurement, absolute predictability demands unvarying uniformity of response to specific stimuli, or in other words, a mechanistic conception of science. In making this demand on psychology the behaviorists will do well to ponder another trend in physical science which is fast displacing the older view that was so complacently accepted as axiomatic. I refer to the theory of indeterminism as opposed to the mechanistic theory so popularly espoused.

A modern physicist has written: "Determinism has faded out of theoretical physics. Some are credulous and can't believe it has been eliminated. Some think it is only a domestic change in physics. Some imagine it is a justification of miracles. Some expect determinism to fade in again" (**12**). Ten years ago practically every physicist of repute was, or thought he was, a determinist. He thought he had discovered strictly causal law. But Eddington, the author of the above quotation, is among those who are reconciled to indeterminism. For him indeterminism is as useful for prediction as determinism. To quote again, "Prediction refers to the average behavior of the vast number of particles. With fewer particles prediction becomes more of a gamble. Backing an electron to do anything is as uncertain as backing a horse." Eddington is not unaware of the argument that this uncertainty may not be due to indeterminism but to ignorance, but until ignorance in the matter has been definitely proven, the physicist can pursue no other course but to go on what he has discovered. Biology, too, has been prevailingly mechanistic, but its predictions also are based upon averages of large numbers of particulars and are not necessarily valid in individual cases. This modern trend in the direction of indeterminism does not, of course, detract from the value of an absolute predictability, but the demand for the latter criterion on the part of the behaviorist cannot disarm the suspicion that in

place of following the lead of the physical sciences he has set up a standard more drastic than physical science is able to maintain. He literally out-sciences science.

Mysticism

Two statements made recently by a psychologist and a physicist respectively are of the utmost significance in representing the opposing trends in these two sciences. In Bentley's recent change of mind he finds it totally unnecessary to assume an organ, power, state or agent called mind or consciousness to account for such processes as thought. The physical organism is all-sufficient for this task. Eddington (10) holds that the substratum of everything is of mental character and he states confidently that no one can deny that mind is the first and most direct thing in our experience, and all else is remote influence. He reaches this conclusion on the ground that modern scientific theories have broken away from the common standpoint which identifies the real with the concrete. He does not go as far as to say that reality is spiritual, but he also does not assume that everything that lacks concreteness is thereby self-condemned (11). Thus each, the psychologist and the physicist, traverses in the progress of his thought the path the other has just left behind. This situation reminds one of the peculiar situation a certain husband and wife found themselves in at the close of their lives. Before their marriage the groom was a staunch Presbyterian and the bride a no less staunch Methodist. During their married life they had many a tilt in which each vigorously expounded the virtues of his and her respective beliefs. So convincing was each in the course of years of argument that at the close of their lives the Presbyterian husband died an ardent Methodist and the Methodist wife died an ardent Presbyterian.

But there is something more than humor in this reversed position of the physicist and the psychologist. For while the psychologist is shrinking from the postulate of consciousness or mind on the ground that it is mystical and cannot be explained scientifically, the physicist flies into the face of mysticism and grasps the postulate abandoned by the psy-

chologist. The physicist is unafraid, and for a good reason. He needs an adequate postulate to account for the new developments in his science. Moreover, mysticism is no hindrance to him, for already he is dealing in it and has done so for a long time. Take, for example, the concept of ether. It is a pure hypothesis. The physicist does not know whether it exists, he has never seen it or experienced it in any way, yet he is willing to postulate it, mysterious as it appears to be, in order to account scientifically for certain physical phenomena. Most astounding of all, the modern physicist has postulated as the most fundamental factor in the universe an energy, electricity, about whose essence he is grossly ignorant. It is a mysterious energy. He can note its effects, but he cannot penetrate the secret of its nature. But who will want to charge the physicist with catering to magic just because he has no adequate explanation for his postulate?

However, the physicist is not the only scientist that recognizes the need of consciousness as a postulate. Certain biologists profess the same need. Herrick (**21**), for example, says that biology is an incomplete science with consciousness left out. Even if the new psychology can get along without it, biology cannot. For Herrick, consciousness is a positive biological factor in animal evolution. Jennings, in his discussion of emergent evolution, calls attention to the utter inadequacy of the physical sciences to account for the phenomena of life, and recognizes the distinctive part played by mental factors in the development of human civilization (**25**). When the physicist and the biologist declare for something in addition to the material in their own respective sciences, the psychologist need not despair of the value and distinctiveness of his science.

In view of what we have here found to obtain in physics and biology, it would appear that consciousness or mind as a form of energy may very well be considered a scientific psychological postulate. It is no more mysterious or magical than the concept of ether or electricity. As in the case of electricity its inner essence is an enigma to us, but the effects of its operation are everywhere discernible, and measurable within limits.

But modern physics has presented us with another important discovery in connection with the break-up of the atom. Not only has the atom been decomposed into energy in the form of electrons and protons, but these forms of energy can operate independently of matter. A distinction has been found between matter and mass. An electron has mass but not matter. Formerly energy was supposed to manifest itself in some movement of matter. Now it is found to be able to exist without matter. This discovery of the physicist is not without possible serious implications for psychology. While it would be unwise to rush to unjustifiable conclusions, one cannot escape the conviction that some of the criticism leveled at mind as a psychological postulate has been seriously weakened. Confessedly, mental phenomena are qualitatively different from physical phenomena, and for their explanation we need a concept that will adequately account for the facts. Mental processes represent a form of energy. In view of the fact that the physicist has invented the terms electron, proton, neutron, photon and positron, why not call this psychological source of energy 'menton'? It can be no more mysterious than electricity. The chief objection to it seems to be that it does not fit into the scheme and methodology proposed for its investigation. Certainly thus far no evidence has been discovered to deny the proposal and no evidence has been discovered or experiment devised that will account for conscious phenomena from a purely physical source, either in biology or physics or chemistry.

Moreover, the theory we propose (not a new one, of course) lends itself to a striking analogy with physics. In radio, for example, a number of elements or factors are essential to the enjoyment of a program; a sending instrument, a receiving apparatus, a medium between the two stations, an auditory organ, a nervous system, consciousness and a subtle, mysterious electrical wave, about whose real nature we know nothing. We can only recognize the effect of its operation. For the appreciation of a radio program all the above mentioned factors must be present and intact. Any break in the series, such as a broken wire, a defective tube, a defective ear, may

render a radio program impossible. The electric current may be generated independently of the other factors, but the result will not be a radio. So by analogy the mind may represent a mysterious energy—no more mysterious than electricity—but to have it appear and function as hearing, seeing, thinking, there is required the medium of nerves, sense organs, physical stimuli, etc.

This theory will probably be called errant dualism, and according to Boring it suffers from or manifests the Cartesian curse, yet in our opinion it is no more unsatisfactory than is Boring's abortive attempt to annihilate it. To say that consciousness or mind is merely a function of the brain is as illogical as it is to say that the radio wave is a function of the sending or receiving sets.

If we now summarize the facts and tendencies we have discovered concerning the postulates in the various sciences, we shall find that in the physical sciences the general trend is

from axiom to hypothesis;
from absolute to relative;
from determinism to indeterminism;
from predictability to probability;
from pure objectivity to a recognition of the subjective.

In psychology on the other hand there is a strong current in the direction of absolute objectivity, mechanism and predictability.

If the ambition of psychology is to be scientific, as is its boast, it will do well to ponder the prevailing trend in the methodology and conclusions of the physical sciences.

REFERENCES

1. Andrade, E. N. De C., The mechanism of nature, London: G. Bell & Sons, 1932, p. 15 ff.
2. ——, ————, p. 125.
3. ——, ————, p. 34.
4. Bentley, M., The new field of psychology, New York: D. Appleton-Century Co., 1934.
5. Boring, G., The physiological dimensions of consciousness, *Science*, 1932, **75**, 32–39.
6. Cunningham, J. T., Modern biology, New York: E. P. Dutton & Co., p. 4.
7. ——, ————, p. viii.

8. DRIESCH, H., The breakdown of materialism, p. 286 ff. in 'The great design,' edited by F. Mason, New York: Macmillan Co.
9. ——, The science and philosophy of the organism, Gifford lectures, 2nd edition, London: A. & C. Black, 1929.
10. EDDINGTON, A. S., The nature of the physical world, New York: Macmillan Co., 1929, p. 258.
11. ——, ————, p. 275 ff.
12. The decline of determinism, Annual report of the Smithsonian Institute, 1932, p. 141 ff.
13. Encyclopedia Brittanica, Foundations of mathematics, Vol. 15, 14th edition.
14. ENRIQUES, F., Problems of science, Chicago: The Open Court Pub. Co., Trans. by K. & J. Royce, 1914, Introduction.
15. ——, ————, p. 173.
16. ——, ————, p. 174.
17. ——, ————, p. 36 ff.
18. ——, ————, p. 38.
19. ——, ————, p. 42.
20. HALDANE, J. S., Mechanism, life and personality, New York: E. P. Dutton & Co., 1914.
21. HERRICK, C. J., Mind and behavior, New York: Van Ostrand & Co., p. 26 ff.
22. HILBERT, D., The foundations of geometry, Trans. by E. J. Townsend, 2nd edition, 1921, Chicago: Open Court Pub. Co., p. 3 ff.
23. ——, ————, p. 130.
24. ——, ————, p. 142.
25. JENNINGS, H. S., The biological basis of human nature, New York: W. W. Norton & Co., 1930, 359–376.
26. LASHLEY, K. S., A behavioristic interpretation of consciousness, PSYCHOL. REV., 1927, 34, p. 433 ff.
27. RAND, B., Modern classical philosophers, New York: Houghton Mifflin & Co., 1924, p. 672 ff.
28. SHARP, L. W., An introduction to cytology, New York: McGraw Hill Book Co., 1934, p. 250 ff.
29. ——, ————, p. 493 ff.
30. THILLY, F., History of modern philosophy, New York: Holt & Co., 1914, p. 505 ff.
31. WATSON, J. B., Psychology from the standpoint of a behaviorist, Philadelphia: J. B. Lippincott & Co., 1919.
32. WILLIAMS, D. C., Scientific method and the existence of consciousness, PSYCHOL. REV., 1934 41, p. 462.

[MS. received May 25, 1935]

STUDIES IN CROSS EDUCATION. V. THEORETICAL

BY THOMAS W. COOK

Acadia University

I. Introduction

The results of experiments on cross-education reported in previous papers (**4**, **5**, **6**, **7**) may be formulated in a number of general statements:

1. A large amount of the gain from direct training of one hand or foot transfers to the other three limbs.

2. The amount of transfer varies with the relative bodily location of the 'practice' and 'transfer' muscle groups. The proportions of transfer from greatest to least are: opposite and symmetrical, same side, opposite and unsymmetrical.

3. The 'absolute' amount of transfer is proportionately greater for 10, 20, 30 and 100 trials initial practice.

4. The 'relative' amount of transfer increases with the perfection of the primary learning.

5. Transfer is approximately equal for mirror-tracing and for learning irregular maze patterns blindfolded.

6. At least for opposite and symmetrical muscle groups, part of the transfer is 'permanent.' The effect of the initial practice is still apparent at the end of a corresponding number of transfer trials.

7. Relative to the rate of learning for the same limb in practice trials, the amount of transfer is about the same from hand to foot and foot to hand.

The purpose of the present paper is to discuss the data upon which these generalizations are based, their relation to former work on cross-education and their significance for psychological and neurological theory.[1]

[1] A review and bibliography of prior work has been given by Bray (**3**). For the reader's convenience the major studies appearing since are listed at the end of this paper.

II. The Experimental Basis of the Laws of Cross Education

1. *Amount of transfer*

With a few minor exceptions probably due to chance errors, all experimenters in the field of cross-education find some positive transfer. The types of learning investigated cover a moderately wide range, and include acts of skill such as mirror-tracing and hitting at a mirror-target, ball-tossing, setting numbers on an adding machine, isolation of control of finger movement, conditional withdrawal to buzzer, 'sensory' transfer in discrimination and transfer of strength gained by exercising with a dumbbell, a dynamometer, or an ergograph. In all the foregoing vision was unrestricted. For 'blindfolded' subjects transfer has been demonstrated by Wieg (**16**) and the writer (unpublished experiment) for regular T- and U-maze patterns, and by the latter for irregular maze patterns (**6, 7**). The evidence for and against the occurrence of negative transfer is discussed later. In general transfer is positive, and in the subsequent discussion 'transfer' signifies positive transfer unless otherwise stated.

Regarding the amount of transfer the data at first sight seem conflicting. Earlier workers give estimates varying from small amounts to almost complete transfer, but with a preponderance of high values. Ewert (**10**), after correcting for what he considers a defect in earlier technique, finds that for mirror-tracing the 'percentage gain' in the idle hand from practice with the other is only 35 per cent for time and 14 per cent for errors (Average Tables 9 to 12). Our experiments yield much larger amounts of transfer, ranging from 60 to 89 per cent (time) for mirror-tracing; 87 to 94 per cent (time), and 80 to 98 per cent (errors) for 'irregular maze' learning. Such disagreement needs explanation.

I have indicated elsewhere that variations in amount of transfer depend to some extent upon the method of computation. (**4**, 158). Regarding the four methods there cited, I stated that "there is no reason to consider one . . . more final or absolute than any of the others." Yet in succeeding papers the third method outlined there has been used exclu-

sively. Is there any justification, apart from brevity and consistency, for considering this anything more than an arbitrary selection from a number of equally good methods? In order to orient the reader my general arrangement of experimental groups and treatment of the data will be outlined briefly. The procedure consists, in the simplest case, in combining the data from the performance of two groups, one having practice trials, for example, with the right hand followed by transfer trials with the left hand, and the other group having practice trials with the left hand succeeded by transfer trials with the right hand. The results of the two groups are combined by averaging, thus equating practice and transfer series for right-left and group differences.[2]

The method is of maximum value in determining percentage of transfer and permanence of transfer. The relation of amount of transfer to amount of practice, the differential transfer to the various muscle groups (except for 100 trials practice), and the comparison of hand-foot with foot-hand transfer, require inter-group comparisons. But where our method of equating is inapplicable, a much larger number of subjects is necessary to obtain reliable results.

The amount (percentage) of transfer is computed from a base obtained by subtracting the average scores on the last practice trial from the average scores on the first practice trial. Dividing this value into the difference between the scores on the first practice and first transfer trial gives 'percentage of transfer.' Dividing 'per cent gain' in the first transfer trial by per cent gain in the practice series gives identical values. The manner in which this consideration of amount of transfer as a function of the amount of initial gain reveals important trends in the data may be illustrated by a hypothetical case. If scores on the first and last of 10 and 100 practice trials, and on the first of the subsequent transfer trials, were as below, the gain in the idle hand from practice with the other would then be 30 per cent for 10 trials and 48 per cent for 100 trials practice. On the other hand, the gain in the idle hand *relative* to the gain in the practiced hand is

[2] A detailed description of the method may be found in earlier papers (4, 5).

60 per cent in both cases. That is, the amount of transfer is directly proportional to the direct gain from practice.

	10 trials		100 trials	
	Practice	Transfer	Practice	Transfer
First trial	100	70	100	52
Last trial	50		20	

Of the other methods outlined (4, 158), number of 'trials saved' was rejected because early and late trials differ so markedly in efficiency as to be quite incomparable. 'Total time saved' is not open to the same objection, but was neglected because it is an absolute rather than a relative measure.

The only difficulty not surmounted by the 'percentage of transfer' method is that it takes account of only the first trial of the transfer series. This is unavoidable, since later trials are influenced by preceding transfer trials. But it means that the reliability of transfer measures depends upon the reliability of a single transfer trial. Hence the need of many experiments and of supplementing measures of 'percentage of transfer' by a study of practice and transfer 'curves.'

In comparing amounts of transfer found by the several experimenters, it is important to note Bray's criticism that "a preliminary test of the unpracticed member . . . makes it impossible to determine the effect of transferred practice on the first trial with the unpracticed part" (3, 451). That this needs reëmphasis is shown by the fact that Munn (14), though acquainted with Bray's work, uses the discarded technique. All previous experiments were arranged as follows: (*a*) One or more trials with one 'hand' (occasionally both hands were trained in the preliminary series), (*b*) practice trials with the other hand, (*c*) end trials with the hand first tested.

Comparing (*a*) and (*c*) gives the amount of improvement due to the interpolation of (*b*). Ewert (10) has pointed out that (*c*) is affected not only by (*b*) but by (*a*). He therefore 'improved' the technique of cross-education experiments by using a control group who took (*a*) and (*c*) but not (*b*), and

deducting the difference between (*a*) and (*c*) for the control group from the difference between (*a*) and (*c*) for the group who had the interpolated (*b*) training. He accordingly finds the gain from transfer much smaller than former data seemed to indicate. As Bray points out, however, Ewert retained a source of error working in the opposite direction to the one he avoided. His 'per cent gain' from transfer is spuriously lowered by excluding the possibility of transfer from the large amount of improvement on the preliminary trial. A more serious defect, however, is introduced by Ewert's method of subtracting the per cent gain made by the control group in one trial from the per cent gain for the practice group for one preliminary trial and a number of practice trials with the other hand. For if the 'training group,' at the end of the first preliminary trial and immediately before the interpolated practice trials, had really reached the degree of efficiency represented by the score of the control group on the second trial (as his procedure assumes), the percentage of 'residual gain' from transfer should be computed from the second trial of the control group and the last trial of the training group. Thus Table 12 shows that Training Group II traces the pattern in 40 seconds on the last trial, and Control Group IV requires 130.4 seconds for their second trial on the same pattern. This gives an 'absolute' gain from transfer of 70 per cent. To find the relation of the gain from transfer to the gain from practice we divide the 88 per cent gain in the practice hand (Table 8) into 70 per cent. The quotient indicates that about 80 per cent of the gain from practice is transferred. Similar treatment of the data in Tables 9 to 11 gives almost identical results. The method of computation is of course not entirely satisfactory, but it suffices to show that Ewert's results do not differ from ours as widely as a cursory survey would indicate.

Bray's data, being free from the defects which he and Ewert find in earlier work, is readily compared with ours. The small number of subjects per group makes comparison of single groups of slight value, but four of his groups were so related that they can be 'paired' and their results averaged

by our method. From Table I of his paper (3, 459) we combine the data for Group 6 which had 10 practice trials with the right hand and 10 transfer trials with the left hand, and Group 7 which had 10 transfer trials with the left hand followed by 10 transfer trials with the right hand. Applying our formula for percentage of transfer (score on the first practice trial minus score on the first transfer trial, divided by score on the first practice trial minus score on the last practice trial), we get $(13.5 - 9.35) \div (13.5 - 8.3)$, or 80 per cent; that is, 80 per cent of the gain from direct training is transferred to the opposite hand. A similar computation for the combined results from Groups 4 and 5 (R. H. practice, R. F. transfer; R. F. practice, R. H. transfer) shows that 70 per cent of the gain from 10 trials practice with the right hand or right foot is transferred to the right foot or right hand. These percentages are quite satisfactorily comparable to ours for 10 trials practice when differences in technique and type of problem are considered.

2. *Amount of transfer and relative position of muscle groups*

Most text-book discussions assume that transfer occurs only between symmetrically related parts of the body. However, not only did one of the first studies (9) indicate that improvement in speed of tapping with the right toe transferred to the left toe, right hand, and right foot, but it should have been evident that a novel pattern of movement traced by one hand, if not too complex, can be duplicated in its main features by any other 'independently mobile' part of the body.[3]

Our results uniformly show not only that training any hand or foot transfers to each of the other hands or feet, but that the amount of transfer is a function of the relative bodily location of the practice and transfer muscle groups. Transfer is greatest to the symmetrical muscle group on the opposite side of the body, next to the same side, and least to the muscle group opposite and unsymmetrical to the part

[3] For fuller treatment see Fox (25, 124) and Lashley (31, 545).

practiced. This relationship holds for all cases tested, *e.g.*, mirror-tracing, 10 trials practice, 77, 66, 55 per cent; 100 trials practice, 89, 81, 74 per cent; irregular maze learning, 90, 80, 63 per cent transfer to opposite and symmetrical, same side, and opposite and unsymmetrical muscle groups respectively.[4]

Comparisons between 'percentages of transfer,' however, only take account of the first transfer trial and should be supplemented by a study of practice and transfer 'curves.' As far as work on transfer has been carried for mirror-tracing, *i.e.*, 10 transfer trials after 10 trials practice (**2**, 695) and 3 transfer trials after 100 trials practice, the muscle groups occupy the same relative positions as on the first transfer trial. For tracing the irregular maze pattern, however, the 'same side' and 'opposite and symmetrical' muscle groups show no differences of importance after the third transfer trial (**6**, 755). Further investigation would be desirable.

Of previous experimenters, only Bray (**3**), Davis (**9**) and Wieg (**16**), give data on amount of transfer to the various muscle groups. Davis's results support ours, but as his differences were small and his groups also small and not equated, I should not attach much importance to this fact. Wieg's findings are also in accord with ours for the only relationship tested. Transfer was greater to opposite and symmetrical than to same side muscle groups. Although her groups numbered only 5 subjects each, they were carefully equated and the advantage of opposite and symmetrical muscle groups hold for both 'the same' and 'opposite' patterns (**16**, 261, Table I). Bray, on the other hand, states that he finds transfer greatest from right hand to right foot. We have indicated, however, that this is true only of the absolute amount gained by foot and hand, and moreover depends for its reliability upon the presumed equality of small groups. On the contrary, when his scores for 10 trials practice are treated by our method, the hand to hand transfer is 80 per cent of the amount learned, while the average transfer

[4] For the original data see (**4**, 153–158), (**5**, 658, Fig. 2), (**5**, 695–698), (**6**, 755, Fig. 2), (**6**, 760–761).

from hand to foot and foot to hand is 70 per cent.[5] Thus in the only case where a comparison is possible, Bray's results agree with ours that the amount of transfer is greater to opposite and symmetrical than to same side muscle groups.

3. *Absolute amount of transfer and number of practice trials*

Our results uniformly show that the 'gross' amount of transfer increases with the number of practice trials. In mirror-tracing the increase is most striking when 10 trials practice is compared with 100 trials practice (**5**, 688, Fig. 3), but it is also indicated clearly by data from transfer following 20 and 30 trials practice (**5**, 688),[6] and from transfer after 10 and 30 trials practice in the experiments on permanence of transfer (**7**, Fig. 2). The results for the irregular maze come from a single experiment (**7**, Table I), but the increase in amount of transfer after 30 as against 10 trials practice is sufficiently great to make it seem probable that the law holds for other types of motor learning as well as mirror-tracing.

When Bray's data (as distinct from his conclusions) are revaluated by our statistical treatment, this is the sole point on which a fundamental disagreement remains between his and our results.[7] He finds no difference in amount of transfer from right hand to right foot after 10, 20 or 60 trials (**3**, Fig. 2). Although in view of the consistency of the results throughout experiments with several hundred subjects, I feel certain that the dependence of amount of transfer upon amount of practice is established for our conditions, there are differences between Bray's and our procedure that make it possible that a repetition of his experiment might not reverse his finding. (*a*) Bray's measure was accuracy and ours (except for the last experiment with the irregular maze) was time. (*b*) The tasks set our subjects were much more complex. On the other hand, the possibility of such a reversal is increased

[5] From our discussion (above) of amount of transfer.

[6] The procedure used in obtaining data for transfer after 20 and 30 trials practice in mirror-tracing is described in (**5**, 687–690).

[7] This only became clear to the writer while preparing the present paper. I was sufficiently misled by superficial disagreements to state in an earlier paper (**7**, 255) that my "results are . . . directly opposed to those of Bray."

by the fact that a function so simple as that tested by Bray would be expected to show very slow improvement after 10 trials,[8] while the mirror-technique makes the data highly susceptible to distortion by a chance upset of the new integration, as the irregularity of many of Bray's curves indicates. It seems probable, therefore, that much larger groups than were used by Bray (10 and 11 subjects for the 10 trial groups) would be needed to discover whether amount of transfer increases with amount of practice in hitting a mirror-target.

4. *Amount of transfer relative to amount of gain in practice series*

If the amount of transfer increased in proportion to the direct gain from practice, the percentages of transfer should be constant from 10 to 100 trials practice. However, our data indicate that the percentage of transfer is greater for the larger amounts of practice. This has been shown by qualitative analysis of individual records for the irregular maze (7, 261–266), but in its quantitative aspect appears most clearly by comparison between the results of the several experiments reported in this paper. For experiments in mirror-tracing performed under closely comparable conditions we find 66 per cent transfer for the average of all muscle groups after 10 trials practice, and 81 per cent for a similar average after 100 trials practice. Again, for slightly different conditions, the percentages of transfer are 76, 90 and 94 for 10, 20 and 30 trials practice. Finally, the last series of experiments shows 60 per cent transfer from 10 trials practice as against 91 per cent transfer from 30 trials practice. With the irregular maze the transfer for the time measure is 87 per cent after 10 trials practice and 94 per cent after 30 trials practice; for the error measure, 80 per cent after 10 trials practice and 98 per cent after 30 trials practice. Thus in every comparable experiment or experiments the percentage of transfer increases uniformly with amount of previous practice.

In an earlier paper (7) I have suggested that the explanation of this phenomenon may lie in negative transfer masking positive transfer if the shift from one muscle group to another

[8] Really, 100 'passes' at the target, since each 'trial' consisted of 10 attempts.

is made after only a few practice trials. If this is the sole cause, positive transfer might be directly proportional to the gain in the practice trials, and the increase in percentage of transfer might be due to the gradual disappearance of negative transfer with increase in number of trials initial practice. Although for irregular maze tracing I have found definite evidence of negative transfer in some individual records, and observation of the subjects' behavior and inspection of their records strongly suggests that negative transfer was present in mirror-tracing, I do not believe that further elaboration of my earlier discussion (**7**) would be of any value until further experiments have been performed. One such project would be to study the relation between percentage of transfer and number of trials practice in types of learning in which negative transfer would seem unlikely to occur.

5. *Type of learning and amount of transfer*

One significant feature of our work is the discovery that transfer is approximately equal for mirror-tracing and for learning the irregular finger maze blindfolded. The range of percentages is indeed slightly higher (63–98) in the latter than in the former (55–94). The smallness of these differences in amount of transfer is of theoretical importance because of the wide discrepancy in the number of features common to practice and transfer trials in the two types of experiment. In mirror-tracing vision was used and verbal formulation of procedure might be supposed to play a part in the learning, whereas in the irregular maze experiment the subjects were blindfolded while tracing and the maze patterns were so irregular as to be extremely difficult to verbalize.

6. *Permanence of transfer*

For 10 and 30 trials practice in tracing a mirror-pattern or an irregular maze pattern the data seem conclusive that transfer persists for an equal number of trials with the symmetrical muscle group on the opposite side of the body (**5**, 698–699; **7**, Figs. 2 and 3). The permanence of transfer to the 'same side' muscle groups rests on one experiment for

10 trials practice in mirror-tracing (**5**, 698; also Figs. 4 and 5). The latter show some 'permanence,' but the differences between the end points of the practice and transfer 'curves' are so small that the data are chiefly valuable because of their consistency with the trend of amount of transfer from greatest to least: opposite and symmetrical, same side, opposite and unsymmetrical muscle groups. For the last named permanence of transfer has not been demonstrated.

Of previous experimenters, the only data on this specific point come from Bray. In Wheeler and Perkins' discussion of Bray's results (**35**, 323), the hand to foot transfer curve alone is reproduced. Since that curve makes it appear that "the practice and transfer curves meet and remain the same after the tenth trial," one might infer that Bray's results show no permanence of transfer. However, if his data are treated in the only way that can give any shadow of reliability for groups of 10 to 14 subjects (*e.g.*, by our method of combining the results of two groups using reversed procedure in practice and transfer trials), we find that this is by no means the case. From the scores for his groups having 10 trials practice (**3**, 459, Table I) I have constructed Fig. 1, in which the left graph shows the average hand to hand transfer (Groups 6 and

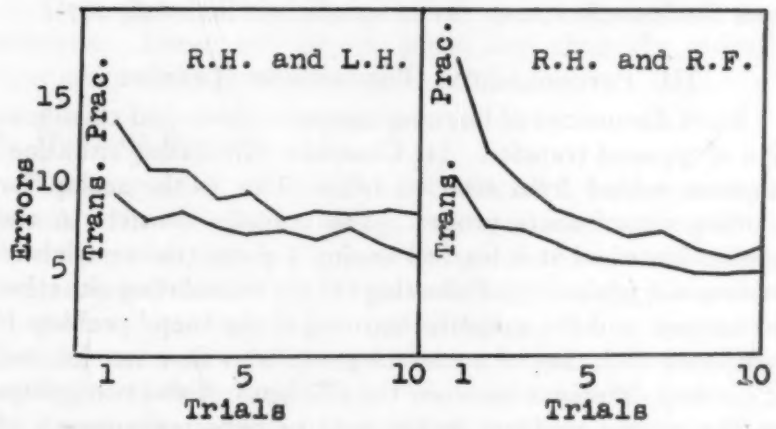

FIG. 1. Analysis of Bray's data to show permanence of transfer after 10 trials practice. Left graph, Groups 6 and 7 (opposite and symmetrical), right graph, Groups 4 and 5 (same side).

7) and the right graph the average right hand to right foot and right foot to right hand transfer (Groups 4 and 5). Both transfer curves are lower than the practice curves throughout, showing permanence of transfer for both opposite and symmetrical and same side muscle groups. For 40 to 60 trials practice Bray tested only hand to foot and foot to hand transfer, and as he does not give averages for more than 10 practice and transfer trials the permanence of transfer cannot be determined from the published data.

7. *Transfer from hand to foot and from foot to hand*

Our results show no consistent differences between hand to foot and foot to hand transfer, nor in transfer from preferred to non-preferred as against non-preferred to preferred hand or foot. This does not necessarily mean that such differences do not exist. It merely indicates that, if present, they are too small to be detected within the range of accuracy of our experiments. In fact, since our method of equating cannot be used, a very large number of subjects would be needed to finally decide the issue. Certainly Bray's finding that transfer from hand to foot is greater than transfer from foot to hand rests on far too few records. It is, moreover, opposed by Wieg's data showing greater transfer when going from the less efficient to the more efficient limb (**16**, 267).

III. Psychological Theories of Transfer

Most discussions of learning assume a three-fold classification of types of transfer. (1) Constant stimulating situation, response varied from trial to trial. This is the set-up for learning experiments proper. The transfer consists in the application of what is learned during a given trial or trials in subsequent trials. (2) Following (1) the stimulating situation is changed, and the subjects' learning of the 'new' problem is compared with that of a control group who have not learned (1). Any difference between the efficiency of the two groups on the second problem is assumed to be a consequence of learning or not learning the first. Such transfer may be positive or negative. If the materials are much alike (*e.g.*, all

mazes or all nonsense syllable lists), the characteristic positive transfer is often called a 'general practice effect.' Otherwise the terminology is that of 'transfer of training' experiments. (3) Again after (1) the stimulating situation is kept constant, and the work is done with a different part of the body. Because bilaterally symmetrical muscle groups were first tested, such transfer has been called cross-education. Intra-muscular or intra-organic transfer might be considered more accurately descriptive, but in the interests of historical continuity the older terminology has been retained throughout the present series of papers.

It should be obvious that learning (1) is basic not only in transfer (2), but also in cross-education. And in the absence of a completely worked out and generally accepted set of generalizations for the theoretical aspects of learning, explanations of cross-education must be fragmentary and inadequate. Theories of cross-education can be no better, at best, than our understanding of the nature of the initial learning. It also follows that since explanations of intraorganic transfer have been formulated in terms of theories used to explain transfer in general, we must examine the basis of these transfer theories and the assumptions required by each, before deciding to what degree each is applicable to cross-education.

Two types of theory have been offered: dualistic and monistic. These will be discussed and then the writer's approach will be presented.

A. Dualistic theories

Under dualistic theories we subsume all hypotheses which postulate two kinds of learning, one of which transfers while the other does not. The concept as thus stated does not imply any particular form of dualism, only that the distinction is qualitative rather than quantitative. As a matter of fact, however, all dualistic theories have been variants of the intelligence-habit dichotomy. The most widely known is the 'generalization' theory of Judd, which assumes that transfer takes place in so far as the subject grasps common relationships implicit in learning and transfer situations. If

the theory is offered as a full explanation, it follows that all learning not due to generalization fails to transfer.

The generalization theory has led to important discoveries in the field of transfer of training, but to date has thrown little light upon the basis of cross-education. Two methods of approach may be distinguished:

(*a*) *Introspective*—The presence or absence of generalization might be studied by asking subjects to observe their mental processes during practice and transfer trials. Thus Munn, in discussing Wheeler's interpretation in terms of insight, says: "If he means that the subject formulates the problem implicitly or explicitly, I must agree with him" (14, 352). I am not satisfied that such a use of introspection is sufficiently accurate to be more than suggestive of possible interpretations, but in any case it has yielded negative results in our work. Only occasionally have I been able to get anything except the most vague and unsatisfactory retrospective accounts of a subject's mental processes during mirror-tracing. Even under conditions more favorable to introspection the results do not indicate that generalization or formulation of methods is a necessary accompaniment of cross-education. Four of five subjects who learned a regular Warden U-maze (unpublished experiment) reported time spent looking for 'a system,' and translation of some part or parts of the pattern into visual imagery. But Subject J, whose transfer from right hand to left hand was 'complete' (within the range of accuracy of the measures), claimed that he neither used imagery of any sort nor verbalized the pattern as a sequence of right and left turns. In this connection it is significant that Lashley found cross-education with monkeys, who could scarcely be said to have formulated the problem—at least in words.

(*b*) Gestalt psychologists use the term 'insight' to refer to certain features of the objective situation. Thus Wheeler quotes Bray's 'discovery' that there is no transfer unless the subject has passed the initial drop in the learning curve and that only the learning from the first 10 trials is transferred, as proof of the insight theory of intraorganic transfer (34, 306).

It is probable that Wheeler would object to being called a dualist, since Gestalt theory purports to be based on a monistic dynamism. Yet it seems to me that his emphasis on the all-or-nothing character and uniqueness of insight has dualistic implications. If only the learning due to insight transfers, there must be a noninsightful kind of learning which does not transfer. His explanation of the incompleteness of transfer in terms of "anatomical and therefore mechanical differences" between the limbs merely avoids the issue. For if the insight gained in the first ten trials alone transfers, what is the nature of the learning in the succeeding 50 trials? It must be a noninsightful learning specific to a particular part of the body.

I would regard Bray's conclusions, if substantiated, as strong evidence in favor of a two-type theory of learning. In fact, I began work on cross-education with a decided bias toward dualism. It seemed possible that the factors conditioning transfer might show a dual pattern, giving proof on the one hand of the existence of a relatively sudden, all-or-nothing type of learning transferring in toto, and of a more gradual, specific kind of learning showing no transfer. But the results have nowhere supported the hypothesis. The variation in amount of transfer to different muscle groups, the permanence of transfer, the absolute and particularly the relative increase in amount of transfer with practice, consistently favor a monistic interpretation. Moreover Bray's own data, when carefully scrutinized, do not justify Wheeler's conclusions. The 'discovery' that subjects who had not passed the first drop in the learning curve showed no transfer is based solely on the records of five subjects, and is thus quite worthless. Nor does examination of the individual records for our subjects reveal any grounds for such a distinction. In this connection I have computed from Ewert's data (**10**) the amount of transfer after *one* trial in mirror-tracing, by averaging the record of Training Group I (L. H. practice, R. H. transfer, Table 9) and Training Group II (R. H. practice, L. H. transfer, Table 10), and find the mean time for the first practice trial to be 220 seconds, for the first transfer trial

196 seconds; average errors, practice, 72, transfer 60. That is to say, there is a substantial amount of transfer from one trial.

I have shown previously that Bray's results indicate permanence of transfer and greater amount of transfer to opposite and symmetrical than to same side muscle groups. The sole point in Bray's data supporting the 'insight' theory of transfer is his failure to demonstrate increase in amount of transfer for more than 10 trials practice. The existence of such an increase is one of the best established facts in our investigation, resting on data from four separate experiments with large groups of subjects.

The generalization or insight interpretation of cross-education is thus of dubious value. The possibility is not excluded that insight transfers as a whole and that the quantitative aspects of transfer are due to the presence of other forms of learning, but neither generalization nor insight can be a complete explanation of transfer.

Regarding the significance of the data from cross-education for the validity of a two-type theory of learning, it must be remembered that the issue between general points of view cannot be decided by experiments in one field. For massed and distributed practice I have found what seems reasonably strong evidence of a dual pattern of factors (**23**), and if further research in other fields substantiates the division of learning into 'achievement' and 'fixing,' it is not difficult to see how the data on cross-education might be reinterpreted to fit a dualistic pattern. I have at present, however, no taste for such a task, since my results can be most satisfactorily formulated in a single interrelated set of quantitative laws rather than qualitative distinctions between types of learning.

B. Monistic theories

1*a*. *Identical elements, theories and transfer*—The theory of identical elements has a psychological and a neurological form; only the former concerns us here. There is of course nothing in the identical elements theory that precludes a dualistic interpretation. There might be two qualitatively

different kinds of elements, one transferrable and the other non-transferrable. But as a matter of history the theory has been the property of the Thorndikian school, and as such thoroughly monistic. Application of the identity hypothesis in explanation of transfer is simplicity itself. All one needs to do is to emphasize similarities (in situations, in mental processes, or in neural bonds) when transfer is found, and to stress differences when there is no transfer. Plainly in order to have any value for research a theory must be more incisive than this.

The only formulation of the identical elements theory that seems to promise anything for interpretation of cross-education has been made by Bruce (**21**) on the basis of his own and earlier work. Bruce states that negative transfer occurs when the stimulus is unchanged and the response changed, while the reverse gives positive transfer. He finds his formula applicable not only to the results of his own carefully controlled experiment, but to the findings of Hunter (**28**), Pearce (**32**) and Wylie (**36**) in simple discriminative maze learning (with rats as subjects), Kline (**30**) with paired words, and Bair (**17**) for card-sorting. It is evident that the hypothesis has some range of application. Can it be elevated to a general principle?

(*a*) The first difficulty is encountered in learning experiments. There, as we have pointed out, the 'situation' is constant and the change in the subject's reactions constitutes the learning. If by stimulus Bruce means the experimental set-up confronting the subject, and by response the subject's reations, the formula will not apply to motor learning. For such learning consists in making a new response to an old stimulus, the relationship which he finds to give negative transfer.

The defect responsible for the failure of Bruce's hypothesis at this point should be evident to anyone familiar with Dewey's discussion of the 'reflex arc.' It consists in the fact that a stimulus is never constant and cannot be constant in motor learning—or indeed in any form of learning where improvement involves a continuous change in the subject's

reactions. If the stimulus were constant, the learning could not take place since the effective stimulus for improvement from trial to trial is the relation between preceding responses and the 'objective' situation. Thus in shooting at a target the second shot will not (on the average) be better than the first unless the subject sees how and where he aimed wrongly.

This limitation considerably narrows the range of Bruce's generalization. In motor learning and problem-solving it applies only to what Gulliksen calls 'transfer of S-R connections' as contrasted with 'transfer of ability to learn' (**27**). That is to say, it applies only to the first transfer trial.

(*b*) A second limitation appears when we attempt to relate the hypothesis to the phenomena of cross-education. For here as well as with initial learning the stimulating situation is 'constant' and the response is novel, in the sense that a different part of the body is used in the transfer trials. Of course the use of a different part of the body involves some change in the stimuli, in the same manner but to a greater degree than from trial to trial in the practice series. The stylus (or what have you) is moved by a different limb, the bodily posture is different, and the mental set is changed. Yet there is reason to believe that even if these were equalized the results would not fit Bruce's formula. In fact, the set-up in Kline and Kohler's experiment on conditioning heel, toe and chin to electric shock (**13**) approximates Gulliksen's 'transfer to S-R connections.' Yet in their work the percentage of reciprocal transfer was high. In any event it is clear that we must part company with Bruce if we use the identity in the stimulating situation to explain positive transfer in cross-education, for according to Bruce stimulus identity with response change gives negative transfer.

(*c*) Even as an interpretation of the data from 'transfer of training' experiments Bruce's hypothesis is subject to many qualifications. I shall only attempt to indicate some of the most important of these, since an exhaustive treatment of transfer problems is outside the scope of this paper.

(*w*) Many important aspects of transfer are not covered by the formula. Thus part of the original task consists in

learning not to react to certain features of the situation, and in developing attitudes, modes of attack, etc.

(*x*) In none of the experiments were the stimuli absolutely different. Certainly they were not in Bruce's experiment, since all the material consisted of nonsense syllables. Thus the positive transfer was not to a 'new' stimulus, but to a situation partly like and partly unlike the original.

(*y*) Strictly applied, Bruce's postulate fits only the paired associates technique. All situations for which his generalizations hold unambiguously consist of only two terms. His own experimental material was paired nonsense syllables; Kline used paired words; Hunter, Wylie, and Pearce tested discrimination between two stimuli mediating a single right-left choice. This limitation resolves the conflict found by Boring (**19**, 207) between Bergström's discovery of interference in card-sorting and the facilitation in 'mediate association' in the work of Ebbinghaus. For card-sorting is essentially a paired-associates technique. The only 'associations' are between a given symbol on a card and a particular box. The order of the appearance of symbols varies 'at random' from trial to trial. It follows that Bruce's formula is wholly applicable. The stimuli (symbols on the cards) are identical in practice and transfer trials. The responses (positions of the individual boxes, which of course includes the particular movement in response to a given symbol) are changed in the transfer series. This should (as it does) lead to interference. But in the Ebbinghaus experiment we have a different situation. The order of the items is constant, and part of the learning consists in relating each item to all parts of the total pattern. Or, in terminology more acceptable to associationists, the earlier items are at least weakly connected with all other items, during the initial learning.

(*z*) There are difficulties in Bruce's formula, however, due in part to the fact that stimuli have pattern characteristics which cannot be resolved into elements without destroying their character. For simultaneous stimulation this has been emphasized by Gestalt psychologists. For successive 'association' it is readily illustrated from an experiment by Hunter.

The ground plans of his pencil mazes (**28**, 53) have two pattern characteristics. The choices are alternately right and left, and the transfer maze (obtained by rotating the practice maze 180 degrees) is the opposite of the first. Comparing the two patterns choice by choice they are absolutely antagonistic, and we might expect a large amount of negative transfer from one to the other. On the contrary, Hunter finds marked positive transfer. The median number of trials for his 31 subjects on the first maze ('habit no. 1') is between 8 and 9 trials, while for the second maze more than half of his subjects required only one trial. How can this positive transfer be explained? Certainly not altogether by identity of elements. No doubt the mazes are alike in all respects except the opposition of the individual choices. But the difficulty for the identity theory comes from the fact that it is precisely in that absolute difference that one important similarity resides. Taken as individual choices the patterns conflict throughout. Taken as total patterns there are two likenesses which depend upon the regularity of the opposition of the elements: both are 'alternating' and one is the opposite of the other.

One reason for ambiguities arising in wider applications of Bruce's hypothesis lies in his definitions of the terms 'stimulus' and 'response.' Bruce, following Carr's terminology (**22**, 102), uses 'stimulus' to mean anything (physical, mental, neural) that is the starting point of a psychological process. Robinson, however, has pointed out that the term stimulus has been specialized for a more restricted use (**33**, 29). To avoid confusion, Robinson substitutes Hollingworth's 'instigating' and 'instigated' items for the Carr-Bills-Bruce 'stimulus' and 'response,' when he is referring to the first and second terms of a pair of associates. In spite of Boring's criticism (**20**), I regard this as a much-needed clarification of terminology. Neither is it a mere matter of choice of words. The Robinsonian definition of stimulus is so firmly intrenched in our thinking that even advocates of the less restricted usage change from one meaning to the other without recognizing the fact. Thus Bills (**18**, 282) follows Robinson when he states

that identity of elements "might refer to the stimuli, and mean that the *material* learned contained identical elements." Here materials are stimuli only. But in Bruce's terminology (which Bills quotes with approval) the first associate of a pair is the stimulus, the second associate a response. Now both stimulus and response are in the material, or, if you like, both are in the subject's reaction. The dual meaning of stimulus and response makes it even more difficult to generalize the data from Hunter's experiments (**28**). Hunter taught rats to turn to the right for light, and to the left for darkness, and found that the 'habit' markedly interfered with subsequent learning to turn to the right for darkness and to the left for light. Here the stimulus is (presumably) the maze with its changing pattern of light and darkness, and the response the rat's behavior. A moment's survey, however, will convince us that this usage of stimulus and response differs widely from that of Bruce. The light (or darkness) may be analagous to the first of a pair of nonsense syllables, but the parallel to the second associate is not the rat's behavior, but (further) stimuli from the maze itself. If we follow Robinson and call the first syllable and the light instigating items, and the second syllable and the right turn (or the food) in the maze instigated items, the ambiguity disappears.

On the whole, we find the identical elements theory of very limited value. Apart from the 'paired associates' technique, its positive contributions may be summed up in three propositions:

I. There can be no transfer without similarity between situations or responses.

II. The degree of similarity sets limits to the amount of transfer.

III. Other things being equal, the amount of transfer is a function of the degree of similarity.

These propositions are almost certainly true, though of course the third need not mean that the relation between similarity and amount of transfer is a simple one. It may be highly complex, containing positive and negative terms, and varying with a wide range of relationships to other factors.

It must be reëmphasized, also, that similarity is not identity. Two things may be similar in pattern without containing any identical 'elements.' It is further evident that if the theory is to be reformulated in terms of similarities (relational patterns) rather than identities (elements), it will not remain within the framework of the Thorndikian or any other atomistic psychology. In fact, it becomes nothing more than one of the general postulates of scientific research: all explanation depends upon the discovery of *significant* similarities between different situations.

1*b*. *The identical elements theory in cross education experiments*—It might be in order to discuss some attempts by investigators of cross-education to find identical elements to which the transfer could be ascribed. Obviously it is useless to seek for an explanation in the material used, for that is constant throughout the experiment (both as regards instigated and instigating items). Regarding identities in the subject's responses, or, more properly, in his S-R 'bonds,' investigators have often been content to point out the large amount of 'sensory' and 'postural' similarity in the learning and transfer series. Thus in almost all experiments vision and the associated eye-movements are used in both practice and transfer trials, and it is possible to observe a general similarity of postural set in both series. Since such 'identities' are often present, their validity as explanatory factors can only be verified or disproved by testing for negative cases. Superficial similarities without causal relationships are not uncommon, but the occurrence of transfer in the absence of any given identity or similarity would be crucial negative evidence.[9]

Wieg's investigation (**16**) and two of the writer's experiments fulfil these conditions (**6**, **7**). For these experiments a 'visual' explanation is excluded, since the subjects were blindfolded, and as different muscles were necessarily used for tracing, it is extremely improbable that there were any sensori-

[9] Not necessarily evidence that the similarity or identity is not significant, but certainly that it is superficial, *i.e.*, that we must seek further in order to find basic explanatory factors common to all situations in which cross education occurs.

motor arcs common to practice and transfer trials. Our results also seem to indicate that it is easy to overestimate the importance of the postural factors. For example, in Experiments I and II the subjects sat for foot trials and stood for hand trials. Yet the hand-foot and foot-hand transfer was about the same as in Experiment III, in which the sitting posture was used throughout.

A simple illustration will serve to show how little these superficial 'identities' really contribute to the solution of the problem. If I shut my eyes and trace a simple irregular figure with my right hand, I can immediately afterwards repeat its general outline with my left hand, either foot, or by walking across the floor. To what sort of 'identity' is this to be attributed?

The only experimental attacks on the problem have been made by Bray (**3**, 460–6) and Norcross (**15**). Since both used much the same method, we need only consider Bray's procedure. He decided from general considerations that part of the transfer might be attributed to "mere familiarity with the setting," and "elimination of the nervous attitude characteristic of the subjects." From his subjects' introspections he further concluded that 'development of methods' was a major factor. He accordingly gave two groups (8 and 9) special training in development of methods by verbal explanation of the mirror effect, of the kinæsthetic method of correcting errors, etc., illustrating the instructions by the behavior of a swinging pendulum seen directly and indirectly in the mirror, and by a trial in the regular apparatus with the experimenter as subject. Groups 8 and 9 subsequently did better in hand tracing than control groups who had no preliminary training in 'development of methods,' but not so well as did groups trained with the opposite hand.

The type of analysis followed by Bray (and Norcross) has some value, but is subject to at least two important limitations. (1) It depends upon the assumption that the psychological activities involved in the 'methods' and 'cross-education' groups are actually identical. (For example, that Bray's subjects actually formulated the problem for them-

selves during tracing in the same way that it was explained to the other group, or that they reorganized their perceptual patterns in identical fashion while watching or tracing.) These assumptions may be based on fact, but their correctness depends upon the experimenter's a-priori analysis and the introspective reports of the subjects, and *upon nothing else.* The method itself cannot be founded on a general principle, for such a principle would read: "If subjects improve while using a given method, all improvement is due to that method." Such a formulation would lead to the absurdity that because the sight of a maze pattern aids later blindfolded tracing, transfer from previous 'blindfolded' trials must be due to the use of vision. (2) Bray's technique is essentially similar to that used in the experiments on 'guidance' at the University of Chicago. Now, if we examine the results of guidance experiments, we find that they differ from the data on cross-education in one important respect. The value of guidance is limited to the first few trials. Larger amounts of guidance (formulation of principles, watching, putting through the act) are of no additional value and may even increase the difficulty of subsequent learning, (**18**, 248). On the contrary, for cross-education our data prove conclusively that (under our conditions) transfer is greater from large than from small amounts of practice. It will be remembered that this is the one point on which we find a fundamental disagreement between Bray's and our results. The theoretical importance of the issue would justify a repetition of Bray's experiment with larger groups of subjects.

2. *Dynamic relationships*—One dualism in scientific thinking has been particularly troublesome for psychology: stability versus change. In order to think at all one must have stable points of reference—units which remain constant at least throughout a given discussion. On the other hand, scientific description tends to be formulated more and more in terms of ever-shifting patterns of dynamic relationships. In the objective sciences the process nature of things is not immediately evident. Rocks, trees and persons seem to be relatively stable wholes, and their description by physics in

dynamic patterns has a strange air of unreality to one used only to the world of 'common sense.' Correspondingly, sensing and perceiving (mental processes most immediately in contact with the external world) seemed to be describable in terms of fixed and stable entities (sensations, etc.). Even here dynamic concepts have always had some place (as in discussions of depth perception), but it remained for Gestalt psychologists to formulate a thorough-going 'perceptual' dynamism.

In the psychology of learning the conflict has been particularly evident. In general the associationists (among whom we include Thorndike, Pavlov and most Behaviorists) have postulated one or another kind of units supposed to remain constant throughout learning. Of all varieties of associationism, the Thorndikians have been most uncompromising opponents of dynamism. Not only are their units unchangeable, but they admit only one type of relationship between those units, the formation of 'bonds.' [10] More eclectic members of the association school have made various compromises with dynamism. Pavlov's research is now described by himself and his co-workers by a system of interrelated variables that bears little resemblance to the original S-R connections. Hull (**29**) is performing a similar service for the Conditioned Response theory in more complex forms of learning. Robinson has expanded 'association' to mean "the establishment of functional relations among psychological activities" (**33**, 7). Here nothing remains of the 'bond' hypothesis except the unchanging character of the things connected.

It is extremely doubtful, however, whether a satisfactory description of the learning process can be made with such a limitation. Learning is change, and the original terms, whether stimuli and responses, or instigating and instigated items, simply do not remain constant in learning. This is the irreconcilable issue between Association and Gestalt views of learning. For if the first item in a list of nonsense sylla-

[10] Thorndike's recent additions to his theory ('belonging,' 'identifiability') have strangely enough led to no qualms regarding their compatibility with the 'bond' hypothesis.

bles changes in character during the memorizing of that 'list,' a formulation of the process in 'bonds,' or even in 'functional relations' between that item and its successors neglects one of the most fundamental facts in the learning.

In the preceding discussion we have found it necessary to introduce dynamic concepts, both for relations between items and for changes in the nature of the items themselves. And it is not improbable that the data will be found to fit naturally and without distortion only into a wholly dynamic description of psychological phenomena such as Gestalt psychologists hope to attain.

C. The present approach

Although the cross-education data seem most readily organized on dynamic principles, neither the present state of Gestalt theory, the work on cross-education, or the writer's insight are sufficiently advanced to warrant the attempt. It is obvious that Wheeler's 'laws of organismic behavior' (**24**, 3–22) represent only the beginning of such a project. Most of these 'laws' are so vaguely delimited and so difficult to submit to crucial experimental test that they represent little more than a confession of faith in Gestalt principles. Our program is much more modest. It consists in attempting to discover some factors in cross-education, and comparing those factors with the conditions governing other forms of learning. Of first importance in this program is the relation of transfer to the initial learning. To what extent is transfer a reproduction of the primary learning? It is evident that learning cannot explain transfer without some analysis of its constituents, for the transfer is incomplete. But it is legitimate to ask: Is the cross-education merely a reduced reproduction of the original learning, retaining all essential characteristics to a less degree, or does it accentuate certain features and minimize others? In favor of the former hypothesis we note that all kinds and degrees of learning seem to transfer, early as well as later acquisitions, misconceptions as well as improvement. Moreover, the transfer is 'permanent'; the transfer curve continues lower than the practice curve

throughout, suggesting that the transferred learning is of a piece with the original. Finally even the negative transfer found in individual records is paralleled by 'reversals' so commonly found in individual records of the early stages of learning. The chief *lack* of correspondence between the laws of original and transferred learning consists in the increase in percentage of transfer with amount of initial practice. It would seem that negative features of the original learning (instability, tendency to reversals) are exaggerated in transfer.

IV. The Physiological Psychology of Cross Education

The writer is inclined to Dunlap's view that much psychological research should precede psycho-neurological speculation (**24**, 53). But something should be said, if only of a negative nature, to relate cross-education to current physiological psychology. In general three problems are raised.

1. The physiological significance of positive transfer, particularly with reference to the difficulties that it raises for the identical (neural) elements theory, has been discussed by Lashley (**31**, 544–5). His conclusions are directly opposed to the hypothesis that during training specific conduction paths are laid down to every effector. The only novel contribution which our studies make to this problem is the discovery that the amount of transfer is as great after learning an irregular maze blindfolded as under conditions more favorable to the theory of the identity of specific paths. In blindfolded maze learning both afferent and efferent neurones are entirely different in practice and transfer trials, and there is no reason to believe that common central neurones are any more necessary than are common peripheral neurones to cross-education. It is possible, of course, that identical neurones might mediate 'supplementary' visual imagery or verbal symbolism, but the irregularity of our patterns, the lack of correlation between reports of common mental processes and amount of transfer, and the inability of subjects who showed a large amount of transfer to give a verbal description of the maze pattern, make it extremely improb-

able that cross-education depends upon identical neural pathways, central or peripheral.

It is true that scientific explanation depends upon the discovery of significant similarities. Cross-education will doubtless ultimately be shown to depend upon some form of neuro-physiological similarity or identity, but there are many possibilities other than a common number of specific neurones.

2. The fact that transfer is not complete has been neglected, usually (as with Lashley) because of preoccupation with the importance of the positive aspect. Most theorists assume that only positive transfer needs explanation. Kline and Kohler, for example, ascribe the mutual transfer from conditioning chin, heel, or toe to a common central pattern (**13**). Yet the universally found residue of untransferred learning raises significant problems. For the 'identity of neurones' hypothesis any such residue is due to non-corresponding neurones in practice and transfer trials. The untenability of this 'neural bond' hypothesis probably justifies Lashley's postulate (**31**, 545) of some such process as equivalence of 'ratios of excitation' in different parts of the cortex. As usually stated, the interpretation of cross-education assumes complete equivalence of such ratios. Just how the theory can be modified to include partial transfer I do not pretend to know. My only purpose here is to point out the importance of the problem.

3. The greater proportion of transfer from the later stages of practice, and the existence of differential transfer to the various muscle groups in the order:—opposite and symmetrical, same side, opposite and unsymmetrical—might be advanced as evidence of diffusion or quantitative spread of neural impulses analogous to Pavlov's 'irradiation,' at least in the form advanced by Gengerelli (**26**). The irradiation hypothesis, however, seems to the writer to depend upon the dubious assumption of localization of learning in specific areas.

REFERENCES

1. BARNES, J. C., Voluntary isolation of control in a natural muscle group, *Psychol. Monog.*, 1916, **22**, No. 93.

2. BEEBY, C. E., An experimental investigation into the simultaneous constituents of an act of skill, *Brit. J. Psychol.*, 1930, **20**, 336–353.
3. BRAY, C. W., Transfer of learning, *J. Exper. Psychol.*, 1928, **11**, 443–467. The citations of original data are by permission of the author.
4. COOK, T. W., Studies in cross education. I. Mirror-tracing the star-shaped maze, *J. Exper. Psychol.*, 1933, **16**, 144–160.
5. ——, ————, II. Further experiments in mirror-tracing the star-shaped maze, *J. Exper. Psychol.*, 1933, **16**, 679–700.
6. ——, ————, III. Kinæsthetic learning of an irregular pattern, *J. Exper. Psychol.*, 1934, **17**, 749–762.
7. ——, ————, IV. Permanence of transfer, *J. Exper. Psychol.*, 1935, **18**, 255–266.
8. CRAFTS, L. W., AND ALLEN, R. M., A comparison of simultaneous and successive methods of motor learning, *Amer. J. Psychol.*, 1934, **46**, 454–461.
9. DAVIS, W. W., Researches in cross education, Stud. Yale Psychol. Lab., 1898, **6**, 18–29; 1900, **8**, 64–109.
10. EWERT, P. H., Bilateral transfer in mirror drawing, *Ped. Sem.*, 1926, **33**, 235–249.
11. GIBSON, T. J., JACK, E. G., AND RAFFEL, G., Bilateral transfer of conditioned response in human subjects, *J. Exper. Psychol.*, 1932, **15**, 416–421.
12. HULIN, W. S., AND KATZ, D., Transfer of training in reading Braille, *Amer. J. Psychol.*, 1934, **46**, 627–631.
13. KLINE, L. W., AND KOHLER, H. M., A comparison of the conditioning of muscular responses which vary in degree of voluntary control, *Amer. J. Psychol.*, 1935, **47**, 129–138.
14. MUNN, N. L., Bilateral transfer of learning, *J. Exper. Psychol.*, 1932, **15**, 343–353.
15. NORCROSS, W. N., Experiments on the transfer of training, *J. Comp. Psychol.*, 1921, **1**, 317–364.
16. WIEG, E. L., Bilateral transfer in the motor learning of young children and adults, *Child Development*, 1932, **3**, 247–268.

ADDITIONAL REFERENCES

17. BAIR, J. H., The practice curve, *Psychol. Rev. Monog.*, 1913, **5**, No. 19, 14–25.
18. BILLS, A. G., General experimental psychology, New York: Longmans, Green, and Co., 1934.
19. BORING, E. G., The physical dimensions of consciousness, New York: The Century Co., 1933.
20. BORING, E. G., *Psychol. Bull.*, 1933, **30**, 454.
21. BRUCE, R. W., Conditions of transfer of training, *J. Exper. Psychol.*, 1933, **16**, 343–361.
22. CARR, H., Psychology, New York: Longmans, Green, 1925.
23. COOK, T. W., Massed and distributed practice in puzzle solving, PSYCHOL. REV., 1934, **41**, 330–355.
24. DUNLAP, K., Habits, New York: Liveright, 1932.
25. FOX, C., Educational psychology, London, 1925.
26. GENGERELLI, J., Brain fields and the learning process, *Psychol. Monog.*, 1935, **45**, No. 203.
27. GULLIKSEN, H., Transfer of response in human subjects, *J. Exper. Psychol.*, 1932, **15**, 496–516.

28. HUNTER, W. S., Habit interference in the white rat and in human subjects, *J. Comp. Psychol.*, 1922, **2**, 29–59.
29. HULL, C. L., The mechanism of the assembly of behavior segments in novel combinations suitable for problem solution, PSYCHOL. REV., 1935, **42**, 219–245. (Contains references to earlier papers on the same topic.)
30. KLINE, L. W., Associative inhibition, *J. Exper. Psychol.*, 1921, **4**, 270–299.
31. LASHLEY, K., Foundations of experimental psychology, Worcester: Clark Univ. Press, 1929.
32. PEARCE, B. D., A note on the interference of visual habits in the white rat, *J. Animal. Behav.*, 1917, **7**, 169–177.
33. ROBINSON, E. S., Association theory today, New York: The Century Co., 1932.
34. WHEELER, R. H., Readings in psychology, New York: Crowell, 1930.
35. WHEELER, R. H., AND PERKINS, F. T., Principles of mental development, New York: Crowell, 1932.
36. WYLIE, H. H., Transfer of response in the white rat, *Behav. Monog.*, 1909, **3**, No. 5.

INTERCORRELATIONS OF MEASURES OF HUMAN LEARNING [1]

BY CALVIN S. HALL

University of Oregon

Is there a general learning ability which enables some individuals to manifest considerable and rapid improvement in a wide range of learning situations and others to perform poorly on the same series of tasks? Or does the term 'improvability' need to be qualified by the specific nature of the task by which improvement is measured, in which case the performance of an individual on one task is but a poor indication of what he will do in another learning situation? Specifically, are the intercorrelations between different measures of improvability of high or low magnitude? [2]

At least nine former investigations in the field of human learning yield evidence on which to base answers to the foregoing questions. A distribution of the correlation coefficients obtained in these experiments appears in Table I. They represent the relationships between a variety of tasks, viz.,

[1] The writer is indebted to Dr. R. C. Tryon and Professor Warner Brown of the University of California for the many helpful suggestions which they gave during the course of the investigation. Professor Howard R. Taylor of the University of Oregon has very kindly read and criticized the manuscript.

[2] The term 'general learning ability' as used in this article is not synonymous with the expression 'general learning factor.' A general learning ability, as indicated by the presence of positive correlation between pure measures of learning, might consist of many independent and uncorrelated unitary factors. In order to ascertain the number of factors which enter into and constitute a given ability or abilities, factor analysis of intercorrelations is customarily employed.

Factor analysis of the intercorrelations reported in this article has not been made for two reasons. The writer believes that measures of the learning process or processes need to be purified experimentally before the results of factor analysis can be satisfactorily interpreted. It will be pointed out that scores made on a learning task are contaminated by many variables extraneous to the learning process. We cannot be at all sure that the relations between learning tasks are due solely to learning ability, for other conditions are present in the learning situation.

Secondly, there is not much to be gained by employing factor analysis when the intercorrelations are so nearly zero, as are those between performances in learning situations.

TABLE I

INTERCORRELATIONS OF SCORES OBTAINED ON LEARNING TASKS

Coefficients taken from Brooks (1), Chapman (2), Garrett (5), Gates (7), Gundlach (9), Haught (10), Heron (11), Race (15), and Thorndike (17)

r	Number of r's
.70–.79	1
.60–.69	4
.50–.59	8
.40–.49	7
.30–.39	9
.20–.29	13
.10–.19	15
.00–.09	14
−.00–.09	5
−.10–.19	3
−.20–.29	4
−.30–.39	0
−.40–.49	1

color-naming, cancellation, opposites, addition, mental multiplication, typewriting, digit-symbol substitution, Turkish-English vocabulary, code learning, rational learning, checker puzzle, stylus maze, inverted writing, number completion, tapping and word building.

The median of the positive coefficients is .25. One-third of the total number of coefficients are significantly different from zero, *i.e.*, the difference between the coefficient and zero being greater than four times the P.E.$_r$, and but thirteen or fifteen per cent are greater than .5. On the basis of these results, therefore, it would appear that a general learning ability, if such exists, is of slight importance in determining learning performance. There would seem to be almost as many learning abilities as tasks by which to measure improvement.

The writer does not believe, however, that these studies constitute a fair test of the problem. He would differentiate between the actual learning process and performance in the learning situation. Learning performance as measured by the score obtained is doubtless contaminated by many factors extraneous to the learning ability or abilities of the subject. A careful reading of the several reports upon which Table I is based suggests a number of such contaminating or extraneous

factors which probably obscure the true relationships between measures of learning ability or abilities. If these irrelevant factors could be eliminated so that pure measures of improvability were obtained, would a general learning ability of some significance then emerge?

I

Irrelevant Factors Which May Raise or Lower Intercorrelations Between Learning Tasks

1. *Range of talent*—It is a well-known statistical principal that the correlation coefficient is systematically lowered by restricting the range of talent sampled [Kelley, (13), Formula 186]. Since all of the studies herewith reviewed employed college students as subjects, it is quite possible that the range of learning talent has been seriously restricted. Were a more representative sample of the total population tested, higher correlations would be obtained. How much the *r*'s would increase is highly speculative since we lack information concerning the variability in learning performance for the total population.

Suppose, however, that intercorrelations were computed on a representative sample of mankind. Would not their very heterogeneity obscure the true relationships between measures of learning as much if not more than the homogeneity which had thereby been eliminated? This, then, is a second possible disturbing factor.

2. *Correlation due to irrelevant factors*—Any unselected group will differ in a great many respects. Some of the more obvious differences will be those of age, sex, nationality, education, socio-economic status and a host of physical and physiological traits. If anyone of these variables be correlated with several learning performances, the intercorrelations between the learning scores will be spuriously increased.

The experimenter who seeks evidence on the problem of general versus specific learning abilities faces a very real dilemma. Too much selection in the group of subjects will lower the relationships between measures of learning, too little selection will raise them because of irrelevant common factors.

Theoretically the dilemma should be resolvable. Practically, it cannot be. If it were possible to measure all of the extraneous variables, possibly their influence could be eliminated by the partial correlation technique. Since it is doubtful if many of the variables can be identified, much less measured, this solution is impractical. The same reasoning applies even more strongly to a second method, that of holding the irrelevant factors constant through proper selection and control.

It may be argued that the lowering of the correlation coefficient due to rigid selection is about offset by the increase in the magnitude of the *r*'s resulting from the unidentifiable and unmeasurable irrelevant factors which escape selection. This, of course, is sheer speculation. To speak baldly, there is no going between the horns of the dilemma. It appears, therefore, that correlations between learning tasks can never be entirely free from some contamination by extraneous factors.

3. *Unreliability of measurement*—Since the fact that correlations between traits are restricted by the reliability of the measures employed has been stressed so often in recent years, the nature of this disturbing factor will not be elaborated here (19). 'True' correlations can be computed by the correction for attenuations formula [Kelley, (13), Formula 155a] which requires for a solution the reliability coefficients of the measures correlated.[3]

Since many of the previous investigators do not cite reliability coefficients for their learning tasks, it is impossible to compute 'true' *r*'s for all of the coefficients appearing in Table I. Taking only those crude *r*'s for which 'true' *r*'s can be calculated and comparing the distribution of crude with

[3] The magnitude of the true correlation coefficients will be affected by the manner in which the reliability coefficients are computed and the particular correction for attenuation formula employed.

Two methods for obtaining reliability coefficients have been popular, the correlation of scores made on alternate forms of the same test separated by an interval of time and the correlations of scores made on odd and even trials or items. Coefficients obtained by the former method are usually of a lower magnitude than those obtained by the latter technique. Consequently, the 'true' *r*'s will be larger if the former method is utilized than if the latter method is employed. For a discussion of methods used in computing reliability coefficients see Spence (16).

corrected *r*'s, an increase in the median value from .29 to .47 is found (Table II). Unreliability of measurement is an im-

TABLE II

A COMPARISON OF CRUDE INTERCORRELATIONS WITH INTERCORRELATIONS CORRECTED FOR ATTENUATION BETWEEN LEARNING PERFORMANCES

Coefficients taken from Garrett (5), Gundlach (9), Race (15), and Thorndike (17)

r	Number of crude *r*'s	Number of *r*'s corrected for attenuation
.90–.99	0	5
.80–.89	0	2
.70–.79	1	2
.60–.69	2	4
.50–.59	6	1
.40–.49	3	4
.30–.39	2	4
.20–.29	8	2
.10–.19	4	5
.00–.09	4	1

portant cause of the low intercorrelations appearing in Table I, and is a factor which needs to be rigorously excluded if true relationships are to be obtained.

4. *Number of subjects*—The stability of the obtained *r*'s is in part a function of the number of subjects used. Correlations based upon fewer than thirty to fifty subjects are inconclusive because of the large P.E.'s and because the usual P.E. formulæ do not hold when applied to a small number of cases, *i.e.*, less than 25. The coefficients reported in Table I are based upon the following numbers of subjects: Chapman, 22; Thorndike, 15; Race, 31, 28, and 32; Garrett, 158; Heron, 93; Brooks, 24; Gundlach, 39; and Gates, 23.

5. *Measure of learning*—Differences in the magnitude of the *r*'s reported in Table I may be due in part to variation in the methods employed for measuring the results of practice. Five of the investigators measured learning in terms of improvement or gain from the initial to the final trial. The remaining four used scores based on the total practice period. It is hardly worthwhile to compare the coefficients obtained from these two kinds of scores since the studies differ in so many other respects. The significance of this factor will be considered in the experiment to be reported below.

6. *Number of trials*—Intercorrelations between scores on a battery of learning tests are affected by the number of trials on which the scores are based. The correlation between two tasks worked on for five trials will be lower than the correlation for the same tasks worked on for ten periods (12). This condition prevails even when the factor of increasing reliability with practice is removed by correcting the crude *r*'s for attenuation (9).

7. *Types of learning tasks used*—In order to test fairly for the existence of a general learning ability, a representative sample of learning tasks should be administered to a large group of subjects. Who knows what a representative sample really is? And would not such a sample, if administered, exhaust the patience of both subject and experimenter? To the first question the answer is undoubtedly 'No one', to the second 'Very likely.'

Types of learning are recognized. For example, there are motor learning, sensory learning, substitution or association learning, tool learning, spatial learning, verbal learning and so forth. Perhaps it would suffice to take a single representative from each of these broad categories were there agreement as to the representativeness of the selected task. In lieu of reliable criteria of the kinds of learning which his tasks measure, the experimenter is forced to use his own judgment and to hope that he samples widely.

Five of the thirteen coefficients greater than .5 in Table I may reasonably be attributed to similarity of content in the learning tasks. Three of them are for different forms of a cancellation test. Two of them are for stylus maze learning.

By judicious sampling of learning tasks with very similar content, it should be possible to obtain *r*'s of a high magnitude, but one could not argue therefrom for a general learning ability. Contrariwise, one could make out a very good case for specialized learning abilities if low correlations were obtained between very similar learning tasks. Heron (11) has reported the following coefficients for maze learning: .23, .22, .42, .39, .35, .11, .11, .50, .31 and .65. These relationships are not impressively high. Unfortunately the reliability of the

mazes is not divulged and attenuating factors may account for these low *r*'s. Tryon (**20**) has reported a coefficient of .8 between rat mazes, and Commins et al (**3**) find 'true' correlations of .65, .56 and .66 between rat mazes. R. L. Thorndike (**18**) has recently reported correlations (corrected for attenuation) of .49, .86, .44, .32, .48 and .71 between various rat mazes. It would appear as though a rather important general maze learning ability did exist. Four coefficients between cancellation tests—.66 reported by Chapman (**2**), .58, .72, and .47 reported by Race (**15**)—testify to a general cancellation learning ability. An *r* of .66 reported by Garrison (**6**) between an eight-letter and a ten-letter rational learning test suggests a general rational learning ability.

The complexity or simplicity of the tasks employed in an investigation of the inter-relations between learning tasks may bias the results either in favor of or against a general ability. Gates used the following simple tasks: color-naming, tapping, addition, multiplication and word building, and obtained a set of inter-correlations none of which differed significantly from zero. The actual range was from −.21 to .18. Gundlach, employing three very difficult and complex tasks, according to his own evaluation, obtained the following intercorrelations: .64, .55 and .55.

A critical test of the existence of a general learning ability would consist, therefore, of an investigation employing a sample of learning tasks representative as to content and complexity. The composite results of the nine studies appearing in Table I are fairly representative did they but satisfy all other conditions of a fair test of the existence of a general learning ability.

8. *Previous practice*—If one individual has had one unit of previous practice on Task A and two units on Task B, and a second individual has had two units of previous practice on A and one unit on B, their relative initial position on the two tasks will be reversed. Should this condition of varying amounts of earlier training prevail for all subjects on the same task and for the same individual on different tasks, the correlations between the several measures will be lowered. Previous

practice will raise the relationships if the amounts of previous practice on the several tasks are constant for the same individual but variable for the group. The former condition seems to the writer to be the more likely to prevail.

Numerous attempts have been made to devise tasks which will eliminate the influence of previous training, either by starting all individuals at zero ignorance or at some common point on the learning curve. I do not believe that any psychologist would hold that such a task had been invented, although some tasks, *e.g.*, mazes, do admittedly minimize past experience.

9. *Positive and negative transfer of training*—Positive transfer is defined as an increase in efficiency in learning Task B as a result of having learned Task A. Negative transfer is defined as a decrease in efficiency in learning Task B as a result of having learned Task A. How can either type of transfer affect the *r*'s between learning performances?

Assuming that all subjects were equally affected by transfer, the mean performance on the second task would be higher if there were positive transfer, lower if there were negative transfer, than on the first task, but the relative positions of the individuals on the two tests would not be altered. Hence, the correlation between the two tasks would not be influenced one way or the other.

It is more probable, however, that the subjects will be differentially affected by transfer, in which case the true relationships between the learning tasks will be masked. Davis states that "the amount of transfer varies in different individuals and within the same individual under different conditions" (4).

The masking may consist of a spurious raising or lowering of the coefficients. If the amount of transfer to Task B is correlated with the amount of improvement on Task A, then the individual's rank on B will be partially determined by his rank on A. Assuming that there is positive transfer and that the individual who displays considerable improvement on Task A will transfer more than the individual who manifests slight improvement, the magnitude of the correlation between

the two performances will be increased. If the extent of negative transfer is inversely correlated with the gains on Task A, a similar increase in the *r*'s will occur.

If there be no correlation between gains and transfer, the correlations between learning tasks will be lowered.

Positive and negative transfer probably can never be entirely eliminated but its influence may be reduced by the proper spacing of practice periods for the several tasks. Transfer was doubtless a more significant variable in the studies conducted by Chapman, Race, Brooks, Gates and Gundlach in which all of the tasks were practiced during the same period than in Heron's study in which the tasks were learned a week apart.

10. *Understanding of the directions.* 11. *Emotional adjustment to the learning situation.* 12. *Motivation.* 13. *Influence of other extraneous factors on the learning process*—These four factors will be considered jointly since their effects on the relationship between learning tasks are similar. If Individual A understands the nature of the problems better, adjusts more quickly to the novelty of the several situations, is more highly motivated on all tasks, and is systematically influenced by more experiences occurring just prior to the time of the experiments which will facilitate learning, than Individual B, the correlation between their scores on the learning tasks will be greater than would be obtained if the influence of these factors were removed.

A more likely result of failure to control one or all of these factors is that they will have a differential effect on the same individual in different learning situations. Individual A will understand one set of instructions better and another worse than Individual B. He will adjust more easily to one situation than to another. He will take an active interest in learning a maze but be extremely bored by learning a list of nonsense syllables. One day he will come to the laboratory overjoyed by 'good news' and the next he will be all gloom. Consequently his performance will vary from task to task and the correlations between performances will be reduced.

That these factors are of considerable importance in de-

termining an individual's performance must be recognized by anyone who has experimented with learning tasks. Instructions are usually standardized and since ability to understand directions is a variable, some individuals will undoubtedly understand the problem better than others. Any task which requires the blindfolding of the subject, as do stylus mazes, is sure to arouse a definite emotional reaction in some subjects and not in others. Motivation resists control. Finally what experimenter can reach out into the daily experiences of the subjects prior to the experiment and render them constant for all members of the experimental group? It is my opinion, after watching hundreds of subjects learn a variety of tasks, that these four factors, and especially motivation, frustrate any attempt to obtain unadulterated measures of learning ability.

Thirteen factors which influence the intercorrelations between learning performances have been discussed. There are doubtless others. Two of these factors—range of talent and number of subjects—are statistical disturbers; they do not influence the score made by any individual on any single task, but they do influence the magnitude and stability of the r's. The remaining eleven enter into the determination of the learning score; any variation in these factors will result in variation in the learning scores. An individual's learning score may be represented by the following equation: Score on learning task $= f$ (learning ability, irrelevant factors, unreliability of measurement, positive or negative transfer, previous practice, type of material used, measures of learning employed, number of trials, understanding of directions, emotional adjustment, motivation and daily variations).

Assuming that all extraneous variables were eliminated from the learning situation, would the intercorrelations be about unity or would they be little changed from the values appearing in Table I?

If the thirteen variables mentioned above are scrutinized for the direction of influence on the correlation coefficients, it will be found that nine of these factors probably lower the correlation between learning tasks. They are restricted range

of talent, unreliability of measurement, positive and negative transfer, previous practice, understanding of directions, emotional adjustments, motivation and daily variations. One of them—correlation due to irrelevant factors—raises the correlations. Three of them—number of subjects, the nature of the tasks employed, and the way in which the performance is scored—may either raise or lower the intercorrelations.

According to this analysis, then, there is every reason to believe that the intercorrelations would rise should these irrelevant variables be eliminated. In the following section an experiment will be reported in which some, but not all, of these factors have been controlled. In this investigation, the intercorrelations between learning tasks are as low as those appearing in Table I. The reasons for these findings will be considered in the final section.

II

An Investigation of the Intercorrelations Between Learning Tasks

1. *Nature of the learning tasks*—The four learning tasks used in this experiment were selected on the basis of the following criteria: the frequency with which they had been previously used, known reliability, apparent dissimilarity of content in order to sample as widely as possible the various so-called types of learning, tasks which could be scored similarly and tasks of sufficient difficulty to yield wide variability during the several practice periods and of sufficient simplicity to insure improvement during the time allotted. The four tasks selected were a stylus maze, the Peterson rational learning test, a list of nonsense syllables and a punchboard maze. These tasks appeared typical of the various tests which have been used in investigations of learning, although the writer hesitates to say what kinds of learning they measure.

The stylus maze consists of forty-T-units cut into bakelite through which the subject pushed a stylus. The rational learning test requires the subject to associate numbers with letters. A sixteen-letter-form (A through P with which were

associated numbers 1 through 16 paired at random with the letters) was devised. Twenty-two nonsense syllables selected from those nonsense syllables in Glaze's list (8) which had zero or approximately zero-association value were exposed at the rate of one a second. After the exposure of the complete series, the subject was asked to write down as many of the syllables as she could recall. The punchboard maze is a large board into the face of which are bored thirty pairs of holes. The subjects went through the thirty pairs with a stylus, selecting that one of each pair which sounded a buzzer.[4]

2. *The subjects*—One hundred college sophomore women volunteered to act as subjects. Their ages ranged from eighteen years and seven months to nineteen years and five months. As compared with the population at large the group was doubtless very homogeneous.

3. *Distribution of practice periods*—Each subject reported once a week on the same day and at the same hour (with a few exceptions) for four consecutive weeks. One task was practiced each week in the following constant order: punchboard maze, rational learning, stylus maze and nonsense syllables.

4. *Number of trials*—The subjects practiced each task for fourteen trials.

5. *Scoring*—Errors—or, in the case of nonsense syllables, the number right—for each trial were the only records kept. Each subject was allowed to adopt her own rate of speed.

6. *Directions and fore-practice*—An attempt was made to insure that each subject was familiar with the nature of the problem before starting the task. Explicit verbal directions were given and thereafter a short fore-practice on each task was administered. The fore-practice for the punchboard maze consisted of one run through the thirty pairs of holes, without knowledge of the correct one of each pair; before the rational learning test the subject was given a three-letter form to learn; before the stylus maze the subject was shown a part of the maze and allowed to run the stylus through the exposed portion; before the nonsense syllable learning the list of

[4] For a more complete description of any feature of this experiment the reader is referred to my dissertation 'The organization of learning abilities' on file at the University of California Library, Berkeley, California.

syllables was run off. After the fore-practice the subjects were allowed to ask questions concerning any feature that was not clear.

7. *Motivation*—No attempt was made to motivate the subjects beyond establishing rapport. The subjects were asked to do as well as they could on each task.

8. *Measures of learning*—In computing the intercorrelations between the performances, two measures of learning were employed: total errors or number right for the fourteen trials, and improvability as measured by the difference between the score on the first two trials and the last two $(1 + 2) - (13 + 14)$.

9. *Reliability of the measures*—The reliability coefficients for the total error score were obtained by summing the errors made on the odd trials and errors made on the even trials and correlating these scores. The Brown-Spearman formula was applied to give the reliability of the entire fourteen trials.

The reliability coefficients, so obtained, are as follows:

Punchboard maze	$.955 \pm .006$
Rational learning	$.923 \pm .011$
Stylus maze	$.927 \pm .010$
Nonsense syllables	$.990 \pm .001$

The coefficients are in every case above .9 and satisfy Kelley's criterion for tests of individual differences.

The reliability coefficients for gains were obtained by correlating the gain from trial one to trial thirteen with the gain from trial two to trial fourteen, (1–13) vs. (2–14) and applying the Brown-Spearman formula to give reliability coefficients for the gains from trials one plus two to trials thirteen plus fourteen $(1 + 2) - (13 + 14)$.

These coefficients are as follows:

Punchboard maze	$.67 \pm .044$
Rational learning	$.50 \pm .067$
Stylus maze	$.44 \pm .075$
Nonsense syllables	$.87 \pm .017$

These coefficients, with the possible exception of the one for nonsense syllables, reflect the unreliability of measures of gains obtained by subtracting final ability from initial ability.

The first trials of almost any learning task are notoriously unreliable. If the scores made on trial one are correlated with those made on trial two, the following reliability coefficients are obtained for the two trials:

Punchboard maze	.37 ± .085
Rational learning	.48 ± .070
Stylus maze	.32 ± .090
Nonsense syllables	.82 ± .023

The relatively high coefficient for nonsense syllables is probably to be explained by the fact that this is the only task which excludes many of the chance factors even in the initial trials. Performance on the punchboard maze, rational learning, and the stylus maze are largely determined by chance factors during the first trial.

10. *Intercorrelations*—The intercorrelations for total error scores and for improvement are as follows:

	Total errors		Gains
	Crude	Corrected	Crude
PB vs. RL	.38 ± .06	.40 ± .06	−.21 ± .06
PB vs. SM	.28 ± .06	.30 ± .07	.20 ± .07
PB vs. NS	.33 ± .06	.34 ± .06	.03 ± .07
RL vs. SM	.27 ± .06	.29 ± .07	−.02 ± .07
RL vs. NS	.18 ± .07	.18 ± .07	−.12 ± .07
SM vs. NS	.11 ± .07	.11 ± .07	.07 ± .07

None of the coefficients between gains is significantly different from zero and since they vary about zero, some positive and some negative, it was thought unwise to apply the correction for attenuation formula to these values.

The intercorrelations for the total score are all positive. Two of them—RL vs NS and SM vs NS—are not significantly different from zero. The remaining four do not indicate a very large community of function between the performances. Certainly no general learning ability of any considerable importance is manifested in these slight relationships. The magnitude of the *r*'s agree with those appearing in Table I.

A further analysis was made by fractionating the total learning score into successive segments of trials. The inter-

correlations for trials 1 and 2, trials 3 through 6, trials 7 through 10 and trials 11 through 14 are not significantly different from those for the total learning score, with the exception of the correlations for trials 1 and 2 which hover around zero.

III

The Theoretical and Practical Significance of These Results

Thirteen factors which influence the relationships between learning performances were described in Part I. In the following table the writer has attempted to state how these factors operated in the present experiment.

1. Range of talent.	Homogeneous group, range probably seriously restricted. Results in lower correlations than would be obtained on a group more representative of the total population
2. Correlation due to irrelevant factors.	Age, nationality, education held constant. Impossible to state what irrelevant factors operated
3. Unreliability of measurement.	Total score very reliable. Gains unreliable
4. Negative and positive transfer.	Attempted to control by administering tests one week apart
5. Previous practice.	Impossible to control
6. Number of subjects.	Adequate for insuring stable coefficients of correlation
7. Type of material used.	Tasks were quite different in nature
8. Measures of learning.	Total area under learning curve and improvement
9. Number of trials.	Intercorrelations would probably be little changed by the addition of more trials
10. Understanding of directions.	Attempted to make sure that all subjects understood the directions
11. Emotional adjustment.	Sufficient fore-practice given to familiarize the subject with the situation
12. Motivation.	Not controlled
13. Daily variations.	Subject came same hour every week

Some of the factors—unreliability of measurement, number of subjects, type of material used, measures of learning, and number of trials—were so controlled as to constitute a fair test of the existence of a general learning ability. Others—correlation due to irrelevant factors, positive and negative transfer, understanding of directions, emotional adjustment and daily variations—were partially controlled. The two

which the writer believes to be the most potent obscurers of the true community of function between learning *qua* learning —previous practice and motivation—were definitely not controlled.

The present study demonstrates that even though some of the postulated disturbers were eliminated, the relationship between learning tasks is still a very slight one. The median intercorrelation between total scores in our study is about .27 which is almost identical with the median coefficient .25, for the intercorrelations in Table I. This would indicate that were all of the other factors irrelevant to the learning process eliminated, the intercorrelations probably might not rise to any considerable extent. The acceptance or rejection of such a belief depends very largely upon how much weight one places upon motivation and previous practice. The writer weights them very heavily. He believes it highly probable that a general learning ability would appear were one able to devise a set of tasks free from the differential effects of previous practice and in the learning of which motivation could be made the same for all subjects.

I presume that it is of some theoretical importance to demonstrate the existence or non-existence of a general learning ability. If such an ability or trait exists, we ought to be examining its properties, genesis and development. If there are only factors specific to each learning task, then a theory of learning ought to be based upon such a fact. At present our 'laws' of learning tacitly assume that there is a *learning process* and not *learning processes*, that the factors determining behavior in one learning situation are quite similar to those determining performance in another learning situation. I presume most American psychologists would agree that the essential process in learning is the formation of associations. If there should be discovered little relationship between pure measures of learning, it would mean that the processes involved in the formation of associations are quite different (or vary) from task to task.[5]

[5] Leeper has recently suggested "that considerable work should be undertaken on the question of whether there are functionally distinct forms of learning, and if so what they are" (14, p. 8).

From a practical point of view the administration of one or several learning tasks will yield very little information concerning an individual's learning performance in other situations. Where measures of learning are to be used for vocational guidance and employment or educational placement, it would be wise to duplicate in the tests of learning the actual situation into which the individual is to be guided or placed.

Conclusion

Previous studies of the relationships between measures of learning demonstrate that performance in the learning situation is very largely specific to the type of task employed. These results are criticised in the present article on the ground that the subjects' performance was in part determined by factors other than their learning ability.

An original investigation in which some of the factors irrelevant to the learning process were eliminated yields correlations of no greater magnitude than those obtained by former investigators. Although the results of the present and previous studies point to a high degree of specificity in learning, the writer believes that a general learning ability of some importance might be discovered were we able to control the differential influence of motivation and previous practice in the learning situation.

REFERENCES

1. Brooks, F. D., Learning in the case of three dissimilar functions, *J. Exper. Psychol.*, 1924, 7, 462–468.
2. Chapman, J. C., Individual differences in ability and improvement and their correlations, New York: Teachers College, 1914.
3. Commins, W. D., McNemar, Q., and Stone, C. P., Intercorrelations of measures of ability in the rat, *J. Comp. Psychol.*, 1932, 14, 225–236.
4. Davis, R. A., Psychology of learning, New York: McGraw-Hill, 1935.
5. Garrett, H. E., The relation of tests of memory and learning to each other and to general intelligence in a highly selected adult group, *J. Educ. Psychol.*, 1928, 19, 601–613.
6. Garrison, K. C., An analytic study of rational learning, George Peabody Coll. for Teachers Contri. to Educ., 1928, No. 44, pp. 52.
7. Gates, G. S., Individual differences as affected by practice, *Arch. Psychol.*, 1922, 58, pp. 74.
8. Glaze, J. A., The associative value of nonsense syllables, *J. Genet. Psychol.*, 1928, 35, 255–269.

9. GUNDLACH, R., The effects of practice on the intercorrelations of three mental tests, *J. Educ. Psychol.*, 1926, 17, 387–401.
10. HAUGHT, B. F., The interrelation of some higher learning processes, *Psychol. Monog.*, 1921, 30, No. 139, 71.
11. HERON, W. T., Individual differences in ability versus chance in the learning of the stylus maze, *Comp. Psychol. Monog.*, 1924, 2, pp. 60.
12. HOLLINGWORTH, H. L., Correlation of abilities as affected by practice, *J. Educ. Psychol.*, 1913, 4, 405–414.
13. KELLEY, T. L., Statistical methods, New York: Macmillan, 1923.
14. LEEPER, R., The role of motivation in learning; a study of the phenomenon of differential motivational control of the utilization of habits, *J. Genet. Psychol.*, 1935, 46, 3–40.
15. RACE, HENRIETTA V., Improvability; its intercorrelations and its relations to initial ability, New York: Teachers College, 1922.
16. SPENCE, K. W., The reliability of the maze and methods of its determination, *Comp. Psychol. Monog.*, 1932, 8, 1–45.
17. THORNDIKE, E. L., Notes on practice, improvability, and the curve of work, *Amer. J. Psychol.*, 1916, 27, 550–565.
18. THORNDIKE, R. L., Organization of behavior in the albino rat, *Genet. Psychol. Monog.*, 1935, 17, 70.
19. TRYON, R. C., Studies in individual differences in maze ability. I. The measurement of the reliability of individual differences, *J. Comp. Psychol.*, 1930, 11, 145–170.
20. TRYON, R. C., Studies in individual differences in maze ability. III. The community of function between two maze abilities, *J. Comp. Psychol.*, 1931, 12, 95–114.

[MS. received June 22, 1935]

PSYCHOLOGICAL REVIEW PUBLICATIONS

Original contributions and discussions intended for the Psychological Review should be addressed to

Professor Herbert S. Langfeld, Editor PSYCHOLOGICAL REVIEW,
Princeton University, Princeton, N. J.

Original contributions and discussions intended for the Journal of Experimental Psychology should be addressed to

Professor Samuel W. Fernberger, Editor JOURNAL OF EXPERIMENTAL PSYCHOLOGY,
University of Pennsylvania, Philadelphia, Pa.

Contributions intended for the Psychological Monographs should be addressed to

Professor John F. Dashiell, Editor PSYCHOLOGICAL MONOGRAPHS,
University of North Carolina, Chapel Hill, N. C.

Reviews of books and articles intended for the Psychological Bulletin, announcements and notes of current interest, and *books offered for review* should be sent to

Professor John A. McGeoch, Editor PSYCHOLOGICAL BULLETIN,
Wesleyan University, Middletown, Conn.

Titles and reprints intended for the Psychological Index should be sent to

Professor Walter S. Hunter, Editor PSYCHOLOGICAL INDEX,
Clark University, Worcester, Mass.

All business communications should be addressed to

Psychological Review Company, Princeton, New Jersey

THE PSYCHOLOGICAL REVIEW

is indexed in the

International Index to Periodicals

to be found in most public and
college libraries

AMERICAN PSYCHOLOGICAL PERIODICALS

American Journal of Psychology—Ithaca, N. Y.; Cornell University.
Subscription $6.50. 624 pages annually. Edited by K. M. Dallenbach, M. F. Washburn, Madison Bentley, and E. G. Boring.
Quarterly. General and experimental psychology. Founded 1887.

Journal of Genetic Psychology—Worcester, Mass.; Clark University Press.
Subscription $14.00 per yr.; $7.00 per vol. 1,000 pages ann. (2 vols.). Edited by Carl Murchison.
Quarterly. Child behavior, animal behavior, comparative psychology. Founded 1891.

Psychological Review—Princeton, N. J.; Psychological Review Company.
Subscription $5.50. 540 pages annually. Edited by Herbert S. Langfeld.
Bi-monthly. General psychology. Founded 1894.

Psychological Monographs—Princeton, N. J.; Psychological Review Company.
Subscription $6.00 per vol. 500 pages. Edited by John F. Dashiell.
Without fixed dates, each issue one or more researches. Founded 1895.

Psychological Index—Princeton, N. J.; Psychological Review Company.
Subscription $4.00. 400–500 pages. Edited by Walter S. Hunter and R. R. Willoughby.
An annual bibliography of psychological literature. Founded 1895.

Psychological Bulletin—Princeton, N. J.; Psychological Review Company.
Subscription $6.00. 720 pages annually. Edited by John A. McGeoch.
Monthly (10 numbers). Psychological literature. Founded 1904.

Archives of Psychology—New York, N. Y.; Columbia University.
Subscription $6.00. 500 pages per volume. Edited by R. S. Woodworth.
Without fixed dates, each number a single experimental study. Founded 1906.

Journal of Abnormal and Social Psychology—Eno Hall, Princeton, N. J.; American Psychological Association.
Subscription $5.00. 448 pages annually. Edited by Henry T. Moore.
Quarterly. Abnormal and social. Founded 1906.

Psychological Clinic—Philadelphia, Pa.; Psychological Clinic Press.
Subscription $3.00. 288 pages. Edited by Lightner Witmer.
Without fixed dates (Quarterly). Orthogenics, psychology, hygiene. Founded 1907.

Journal of Educational Psychology—Baltimore: Warwick & York.
Subscription $6.00. 720 pages. Monthly except June to August.
Edited by J. W. Dunlap, P. M. Symonds and H. E. Jones. Founded 1910.

Psychoanalytic Review—Washington, D. C.; 3617 10th St., N. W.
Subscription $6.00. 500 pages annually. Edited by W. A. White and S. E. Jelliffe.
Quarterly. Psychoanalysis. Founded 1913.

Journal of Experimental Psychology—Princeton, N. J.; Psychological Review Company.
Subscription $7.00. 900 pages annually. Edited by Samuel W. Fernberger.
Bi-monthly. Experimental psychology. Founded 1916.

Journal of Applied Psychology—Indianapolis; C. E. Pauley & Co.
Subscription $6.00. 600 pages annually. Edited by James P. Porter, Ohio University, Athens, Ohio. Bi-monthly. Founded 1917.

Journal of Comparative Psychology—Baltimore, Md.; Williams & Wilkins Company.
Subscription $5.00 per volume of 450 pages. Ed. by Knight Dunlap and Robert M. Yerkes. Two volumes a year. Founded 1921.

Comparative Psychology Monographs—Baltimore, Md.; The Johns Hopkins Press.
Subscription $5.00. 400 pages per volume. Roy M. Dorcus, Editor.
Published without fixed dates, each number a single research. Founded 1922.

Genetic Psychology Monographs—Worcester, Mass.; Clark University Press.
Subscription $7.00 per vol. One volume per year. Edited by Carl Murchison.
Bi-monthly. Each number one complete research. Child behavior, animal behavior, and comparative psychology. Founded 1925.

Psychological Abstracts—Eno Hall, Princeton, N. J.; American Psychological Association.
Subscription $6.00. 700 pages ann. Edited by Walter S. Hunter and R. R. Willoughby.
Monthly. Abstracts of psychological literature. Founded 1927.

Journal of General Psychology—Worcester, Mass.; Clark University Press.
Subscription $14.00 per yr.; $7.00 per vol. 1,000 pages ann. (2 vols.). Edited by Carl Murchison.
Quarterly. Experimental, theoretical, clinical, historical psychology. Founded 1927.

Journal of Social Psychology—Worcester, Mass.; Clark University Press.
Subscription $7.00. 500 pages annually. Ed. by John Dewey and Carl Murchison.
Quarterly. Political, racial, and differential psychology. Founded 1929.